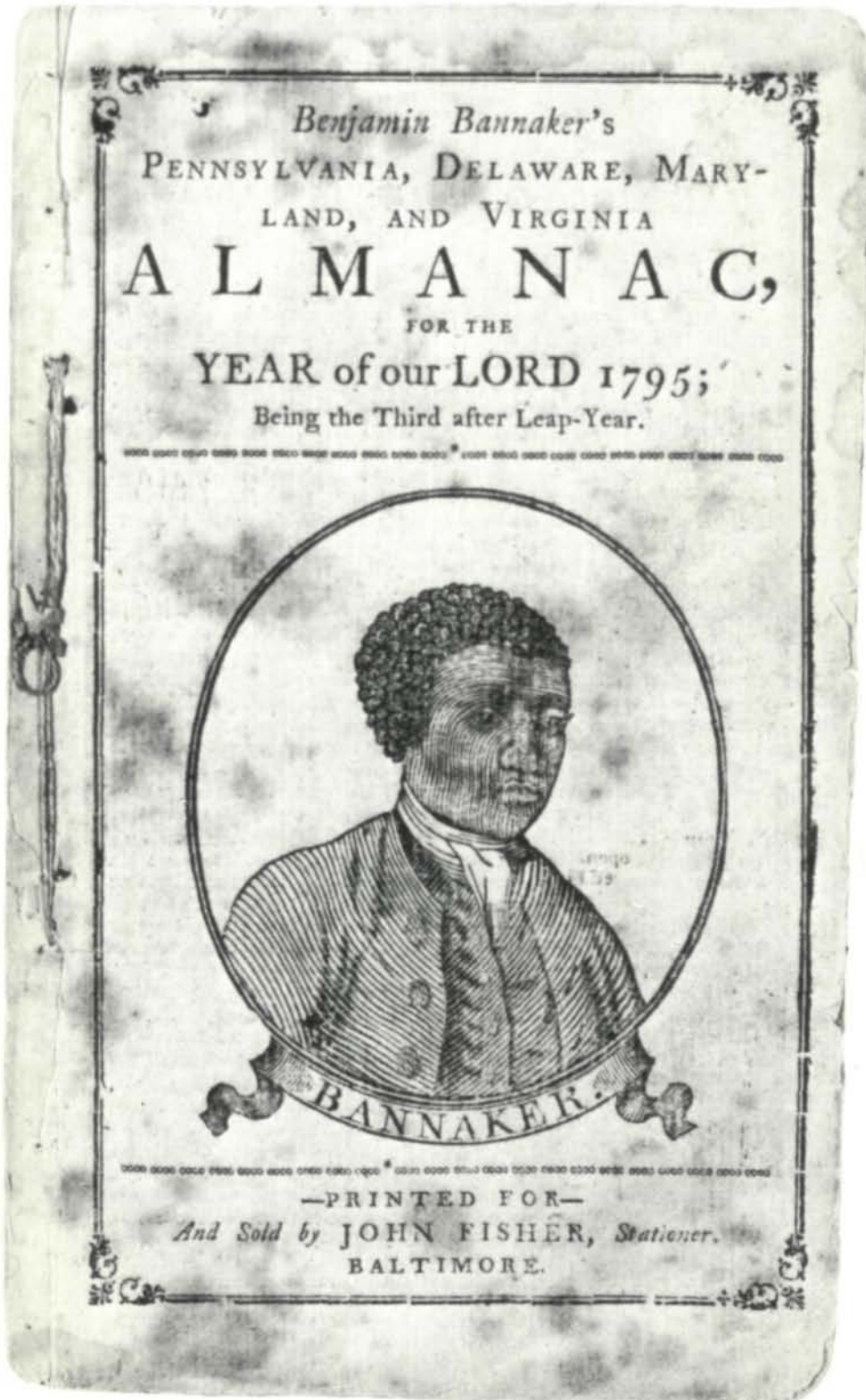


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Mirror for Americans: A Century of Reconstruction History

JOHN HOPE FRANKLIN

PERHAPS NO HUMAN EXPERIENCE is more searing or more likely to have a long-range adverse effect on the participants than violent conflict among peoples of the same national, racial, or ethnic group. During the conflict itself the stresses and strains brought on by confrontations ranging from name-calling to pitched battles move people to the brink of mutual destruction. The resulting human casualties as well as the physical destruction serve to exacerbate the situation to such a degree that reconciliation becomes virtually impossible. The warring participants, meanwhile, have done irreparable damage to their common heritage and to their shared government and territory through excessive claims and counterclaims designed to make their opponents' position appear both untenable and ludicrous.

Situations such as these have occurred throughout history; they are merely the most extreme and most tragic of numerous kinds of conflicts that beset mankind. As civil conflicts—among brothers, compatriots, coreligionists, and the like—they present a special problem not only in the prosecution of the conflict itself but in the peculiar problems related to reconciliation once the conflict has been resolved. One can well imagine, for example, the utter bitterness and sense of alienation that both sides felt in the conflict that marked the struggle for power between the death in 1493 of Sonni Ali, the ruler of the Songhay empire, and the succession of Askia Muhammad some months later. The struggle was not only between the legitimate heir and an army commander but also between the traditional religion and the relatively new, aggressive religion of Islam, a struggle in which the military man and his new religion emerged victorious.¹

Historians have learned a great deal about these events, although they are wrapped in the obscurity and, indeed, the evasive strategies of the late Middle Ages. Despite the bitterness of the participants in the struggle and the dissipating competition of scholars in the field, we have learned much more about the internal conflicts of the Songhay empire of West Africa and about the details of Askia Muhammad's program of reconstruction than we could possibly

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¹ Nehemiah Levtzion, "The Long March of Islam in the Western Sudan," in Roland Oliver, ed., *The Middle Age of African History* (London, 1967), 16–17.

have anticipated—either because the keepers of the records were under his influence or because any uncomplimentary accounts simply did not survive. Interestingly enough, however, the accounts by travelers of the energetic and long-range programs of reconstruction coincide with those that the royal scribes provided.²

Another example of tragic internal conflict is the English Civil War of the seventeenth century. The struggle between Charles I and those who supported a radical Puritan oligarchy led not only to a bloody conflict that culminated in the execution of the king but also to bizarre manifestations of acrimony that ranged from denouncing royalism in principle to defacing icons in the churches. Not until the death of Oliver Cromwell and the collapse of the Protectorate were peace and order finally achieved under Charles II, whose principal policies were doubtless motivated by his desire to survive. The king's role in the reconstruction of England was limited; indeed, the philosophical debates concerning, as well as the programs for, the new society projected by the Protectorate had a more significant impact on England's future than the restoration of the Stuarts had.

Thanks to every generation of scholars that has worked on the English Civil War and its aftermath, we have had a succession of illuminations without an inordinate amount of heat. Granted, efforts to understand the conflict have not always been characterized by cool objectivity and generous concessions. But, because historians have been more concerned with understanding the sources than with prejudging the events with or without the sources, we are in their debt for a closer approximation to the truth than would otherwise have been the case.³

I daresay that both the Africanists concerned with Songhay and the students of the English Civil War will scoff at these general statements, which they may regard as a simplistic view of the struggles that they have studied so intensely. I am in no position to argue with them. The point remains that, whether one views the internal conflicts of the people of Songhay in the fifteenth century, the English in the seventeenth century, or the Americans in the nineteenth century, the conflict itself was marked by incomparable bitterness and extensive bloodshed. The aftermath, moreover, was marked by continuous disputation over the merits of the respective cases initially as well as over the conduct of the two sides in the ensuing years. These continuing disputations, it should be added, tell as much about the times in which they occurred as about the period with which they are concerned. And, before I do violence either to the facts themselves or to the views of those who have studied these events, I shall seek to establish my claim in the more familiar environment of the aftermath of the Civil War in the United States.

² Leo Africanus, *The History and Description of Africa*, 3 (New York, n.d.): 823–25; and Mahmoud Kati, *Tarikh El-Fettach*, ed. O. Houdas and M. Delafosse (Paris, 1913), 13–54.

³ See, for example, Christopher Hill, *Puritanism and Revolution: Studies in Interpretation of the English Revolution of the 17th Century* (New York, 1964), esp. chap. 1; and David Underdown, *Royalist Conspiracy in England* (New Haven, 1960).

IN TERMS OF THE TRAUMA and the sheer chaos of the time, the aftermath of the American Civil War has few equals in history. After four years of conflict the burden of attempting to achieve a semblance of calm and equanimity was almost unbearable. The revolution in the status of four million slaves involved an incredible readjustment not only for them *and* their former owners but also for all others who had some understanding of the far-reaching implications of emancipation. The crisis in leadership occasioned by the assassination of the president added nothing but more confusion to a political situation that was already thoroughly confused. And, as in all similar conflicts, the end of hostilities did not confer a monopoly of moral rectitude on one side or the other. The ensuing years were characterized by a continuing dispute over whose side was right as well as over how the victors should treat the vanquished. In the post-Reconstruction years a continuing argument raged, not merely over how the victors did treat the vanquished but over what actually happened during that tragic era.

If every generation rewrites its history, as various observers have often claimed, then it may be said that every generation since 1870 has written the history of the Reconstruction era. And what historians have written tells as much about their own generation as about the Reconstruction period itself. Even before the era was over, would-be historians, taking advantage of their own observations or those of their contemporaries, began to speak with authority about the period.

James S. Pike, the Maine journalist, wrote an account of misrule in South Carolina, appropriately called *The Prostrate State*, and painted a lurid picture of the conduct of Negro legislators and the general lack of decorum in the management of public affairs.⁴ Written so close to the period and first published as a series of newspaper pieces, *The Prostrate State* should perhaps not be classified as history at all. But for many years the book was regarded as authoritative—contemporary history at its best.⁵ Thanks to Robert Franklin Durden, we now know that Pike did not really attempt to tell what he saw or even what happened in South Carolina during Reconstruction. By picking and choosing from his notes those events and incidents that supported his argument, he sought to place responsibility for the failure of Reconstruction on the Grant administration and on the freedmen, whom he despised with equal passion.⁶

A generation later historians such as William Archibald Dunning and those who studied with him began to dominate the field. Dunning was faithfully described by one of his students as “the first to make scientific and scholarly investigation of the period of Reconstruction.”⁷ Despite this evaluation, he was as unequivocal as the most rabid opponent of Reconstruction in placing upon

⁴ Pike, *The Prostrate State: South Carolina under Negro Government* (New York, 1873).

⁵ See the very favorable comments by Henry Steele Commager in the introduction to a reissue of *The Prostrate State* (New York, 1935).

⁶ Durden, *James Shepherd Pike: Republicanism and the American Negro, 1850-1882* (Durham, N.C., 1957), 214-19.

⁷ Hamilton, “William Archibald Dunning,” *Dictionary of American Biography*, 3, pt. 1: 523.

Scalawags, Negroes, and Northern radicals the responsibility for making the unworthy and unsuccessful attempt to reorder society and politics in the South.⁸ His “scientific and scholarly” investigations led him to conclude that at the close of Reconstruction the planters were ruined and the freedmen were living from hand to mouth—whites on the poor lands and “thrifless blacks on the fertile lands.”⁹ No economic, geographic, or demographic data were offered to support this sweeping generalization.

Dunning’s students were more ardent than he, if such were possible, in pressing their case against Radical Republicans and their black and white colleagues. Negroes and Scalawags, they claimed, had set the South on a course of social degradation, misgovernment, and corruption. This tragic state of affairs could be changed only by the intervention of gallant men who would put principle above everything else and who, by economic pressure, social intimidation, and downright violence, would deliver the South from Negro rule. Between 1900 and 1914 these students produced state studies and institutional monographs that gave more information than one would want about the complexion, appearance, and wearing apparel of the participants and much less than one would need about problems of postwar adjustment, social legislation, or institutional development.¹⁰

Perhaps the most important impact of such writings was the influence they wielded on authors of textbooks, popular histories, and fiction. James Ford Rhodes, whose general history of the United States was widely read by contemporaries, was as pointed as any of Dunning’s students in his strictures on Reconstruction: “The scheme of Reconstruction,” he said, “pandered to ignorant negroes, the knavish white natives, and the vulturous adventurers who flocked from the North. . . .”¹¹ Thomas Dixon, a contemporary writer of fiction, took the findings of Rhodes’s and Dunning’s students and made the most of them in his trilogy on Civil War and Reconstruction. In *The Clansman*, published in 1905, he sensationalized and vulgarized the worst aspects of the Reconstruction story, thus beginning a lore about the period that was dramatized in *Birth of a Nation*, the 1915 film based on the trilogy, and popularized in 1929 by Claude Bowers in *The Tragic Era*.¹²

Toward the end of its most productive period the Dunning school no longer held a monopoly on the treatment of the Reconstruction era. In 1910 W. E. B. DuBois published an essay in the *American Historical Review* entitled, significantly, “Reconstruction and Its Benefits.” DuBois dissented from the prevailing view by suggesting that something good came out of Reconstruction, such as educa-

⁸ Dunning, *Reconstruction, Political and Economic, 1865–1877* (New York, 1907), 116, 120, 121, 213.

⁹ Walter L. Fleming, ed., *Documentary History of Reconstruction: Political, Military, Social, Religious, Educational, and Industrial, 1865–1906*, 1 (New York, 1966): 267.

¹⁰ For some of the best examples of the work of Dunning’s students, see Walter L. Fleming, *Civil War and Reconstruction in Alabama* (New York, 1905); and Joseph G. de Roulhac Hamilton, *Reconstruction in North Carolina* (New York, 1914).

¹¹ Rhodes, *History of the United States*, 7 (New York, 1906): 168.

¹² Dixon, *The Leopard’s Spots: A Romance of the White Man’s Burden* (New York, 1902), *The Clansman: An Historical Romance of the Ku Klux Klan* (New York, 1905), and *The Traitor: A Story of the Rise and Fall of the Invisible Empire* (New York, 1907); and Bowers, *The Tragic Era: The Revolution after Lincoln* (New York, 1929).

tional opportunities for freedmen, the constitutional protection of the rights of all citizens, and the beginning of political activity on the part of the freedmen. In an article published at the turn of the century, he had already hinted "that Reconstruction had a beneficial side," but the later article was a clear and unequivocal presentation of his case.¹³

DuBois was not the only dissenter to what had already become the traditional view of Reconstruction. In 1913 a Mississippi Negro, John R. Lynch, former speaker of the Mississippi House of Representatives and former member of Congress, published a work on Reconstruction that differed significantly from the version that Mississippi whites had accepted. Some years later he argued that a great deal of what Rhodes had written about Reconstruction was "absolutely groundless." He further insisted that Rhodes's account of Reconstruction was not only inaccurate and unreliable but was "the most one-sided, biased, partisan, and prejudiced historical work" that he had ever read.¹⁴ A few years later Alrutheus A. Taylor published studies of the Negro in South Carolina, Virginia, and Tennessee, setting forth the general position that blacks during Reconstruction were not the ignorant dupes of unprincipled white men, that they were certainly not the corrupt crowd they had been made out to be, and that their political influence was quite limited.¹⁵

The most extensive and, indeed, the most angry expression of dissent from the well-established view of Reconstruction was made in 1935 by W. E. B. DuBois in his *Black Reconstruction*. "The treatment of the period of Reconstruction reflects," he noted, "small credit upon American historians as scientists." Then he recalled for his readers the statement on Reconstruction that he wrote in an article that the *Encyclopaedia Britannica* had refused to print. In that article he had said, "White historians have ascribed the faults and failures of Reconstruction to Negro ignorance and corruption. But the Negro insists that it was Negro loyalty and the Negro vote alone that restored the South to the Union, established the new democracy, both for white and black, and instituted the public schools."¹⁶ The *American Historical Review* did no better than the *Encyclopaedia Britannica*, since no review of *Black Reconstruction*, the first major scholarly work on Reconstruction since World War I, appeared in the pages of the *Review*. The work was based largely on printed public documents and secondary literature because, the author admitted, he lacked the resources to engage in a full-scale examination of the primary materials¹⁷ and because DuBois thought of his task as the

¹³ DuBois, "The Freedmen's Bureau," *Atlantic Monthly*, 87 (1901): 354-65, and "Reconstruction and Its Benefits," *AHR*, 15 (1909-10): 781-99.

¹⁴ Lynch, *The Facts of Reconstruction* (Boston, 1913), Preface, 92-99 (this entire volume is reprinted in John Hope Franklin, ed., *Reminiscences of an Active Life: The Autobiography of John Roy Lynch* [Chicago, 1970], xxvii-xxxviii), and *Some Historical Errors of James Ford Rhodes* (Boston, 1922), xvii. The latter work originally appeared as two articles in the *Journal of Negro History*: "Some Historical Errors of James Ford Rhodes," 2 (1917): 345-68, and "More about the Historical Errors of James Ford Rhodes," 3 (1918): 139-57. Also see John Garraty, ed., *The Barber and the Historian: The Correspondence of George A. Myers and James Ford Rhodes* (Columbus, Ohio, 1956), 29-38.

¹⁵ Taylor, *The Negro in South Carolina during the Reconstruction* (Washington, 1924), *The Negro in the Reconstruction of Virginia* (Washington, 1926), and *The Negro in Tennessee* (Washington, 1941).

¹⁶ DuBois, *Black Reconstruction: An Essay toward a History of the Part Which Black Folk Played in the Attempt to Reconstruct America, 1860-1880* (New York, 1935), 713.

¹⁷ *Ibid.*, 724.

exposure of the logic, argument, and conclusions of those whose histories of Reconstruction had become a part of the period's orthodoxy. For this task he did not need to delve deeply into the original sources.

From that point on, works on Reconstruction represented a wide spectrum of interpretation. Paul Herman Buck's *Road to Reunion* shifted the emphasis to reconciliation, while works by Horace Mann Bond and Vernon L. Wharton began the program of fundamental and drastic revision.¹⁸ No sooner was revisionism launched, however, than E. Merton Coulter insisted that "no amount of revision can write away the grievous mistakes made in this abnormal period of American history." He then declared that he had not attempted to do so, and with that he subscribed to virtually all of the views that had been set forth by the students of Dunning. And he added a few observations of his own, such as "education soon lost its novelty for most of the Negroes"; they would "spend their last piece of money for a drink of whisky"; and, being "by nature highly emotional and excitable . . . , they carried their religious exercises to extreme lengths."¹⁹

By mid-century, then, there was a remarkable mixture of views of Reconstruction by historians of similar training but of differing backgrounds, interests, and commitments. Some were unwilling to challenge the traditional views of Reconstruction. And, although their language was generally polite and professional, their assumptions regarding the roles of blacks, the nature of the Reconstruction governments in the South, and the need for quick—even violent—counteraction were fairly transparent. The remarkable influence of the traditional view of Reconstruction is nowhere more evident than in a work published in 1962 under the title *Texas under the Carpetbaggers*. The author did not identify the carpetbaggers, except to point out that the governor during the period was born in Florida and migrated to Texas in 1848 and that the person elected to the United States Senate had been born in Alabama and had been in Texas since 1830.²⁰ If Texas was ever under the carpetbaggers, the reader is left to speculate about who the carpetbaggers were! Meanwhile, in the 1960s one of the most widely used college textbooks regaled its readers about the "simple-minded" freedmen who "insolently jostled the whites off the sidewalks into the gutter"; the enfranchisement of the former slaves set the stage for "stark tragedy," the historian continued, and this was soon followed by "enthroned ignorance," which led inevitably to "a carnival of corruption and misrule."²¹ Such descriptions reveal more about the author's talent for colorful writing than about his commitment to sobriety and accuracy.

Yet an increasing number of historians began to reject the traditional view and to argue the other side or, at least, to insist that there was another side. Some took another look at the states and rewrote their Reconstruction history. In the new version of Reconstruction in Louisiana the author pointed out that

¹⁸ Buck, *The Road to Reunion* (Boston, 1937); Bond, *Negro Education in Alabama: A Study in Cotton and Steel* (Washington, 1939); and Wharton, *The Negro in Mississippi* (Chapel Hill, 1947).

¹⁹ Coulter, *The South during Reconstruction* (Baton Rouge, 1947), xi, 86, 336.

²⁰ W. L. Nunn, *Texas under the Carpetbaggers* (Austin, 1962), 19, 25n.

²¹ Thomas A. Bailey, *The American Pageant: A History of the Republic* (Boston, 1961), 475–76.

“the extravagance and corruption for which Louisiana Reconstruction is noted did not begin in 1868,” for the convention of 1864 “was not too different from conventions and legislatures which came later.”²² Others looked at the condition of the former slaves during the early days of emancipation and discovered that blacks faced freedom much more responsibly and successfully than had hitherto been described. Indeed, one student of the problem asserted that “Reconstruction was for the Negroes of South Carolina a period of unequaled progress.”²³ Still others examined institutions ranging from the family to the Freedmen’s Savings Bank and reached conclusions that were new or partly new to our understanding of Reconstruction history.²⁴ Finally, there were the syntheses that undertook, unfortunately all too briefly, to make some overall revisionist generalizations about Reconstruction.²⁵

UP TO THIS POINT my observations have served merely as a reminder of what has been happening to Reconstruction history over the last century. I have not intended to provide an exhaustive review of the literature. There have already been extensive treatments of the subject, and there will doubtless be more.²⁶ Reconstruction history has been argued over and fought over since the period itself ended. Historians have constantly disagreed not only about what significance to attach to certain events and how to interpret them but also (and almost as much) about the actual events themselves. Some events are as obscure and some facts are apparently as unverifiable as if they dated from several millennia ago. Several factors have contributed to this state of affairs. One factor, of course, is the legacy of bitterness left behind by the internal conflict. This has caused the adversaries—and their descendants—to attempt to place the blame on each other (an understandable consequence of a struggle of this nature). Another factor is that the issues have been delineated in such a way that the merits in the case have tended to be all on one side. A final factor has been the natural inclination of historians to pay attention only to those phases or aspects of the period that give weight to the argument presented. This inclination may involve the

²² Joe Gray Taylor, *Louisiana Reconstructed, 1863–1877* (Baton Rouge, 1974), 49.

²³ Joel Williamson, *After Slavery: The Negro in South Carolina during Reconstruction, 1861–1877* (Chapel Hill, 1965), 63. Also see Roberta Sue Alexander, “North Carolina Faces the Freedmen: Race Relations during Presidential Reconstruction, 1865–1867” (Ph.D. dissertation, University of Chicago, 1974).

²⁴ For examples of such work, see Herbert G. Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976); John W. Blassingame, *Black New Orleans, 1860–1880* (Chicago, 1973); and Carl R. Osthaus, *Freedmen, Philanthropy, and Fraud: A History of the Freedmen’s Savings Bank* (Urbana, Ill., 1976).

²⁵ Rembert Patrick, *The Reconstruction of the Nation* (New York, 1967); Kenneth M. Stampp, *The Era of Reconstruction, 1867–1877* (New York, 1965); and John Hope Franklin, *Reconstruction after the Civil War* (Chicago, 1961).

²⁶ See, for example, A. A. Taylor, “Historians of the Reconstruction,” *Journal of Negro History*, 23 (1938): 16–34; Francis B. Simkins, “New Viewpoints of Southern Reconstruction,” *Journal of Southern History*, 5 (1939): 49–61; Howard K. Beale, “On Rewriting Reconstruction History,” *AHR*, 45 (1939–40): 807–27; T. Harry Williams, “An Analysis of Some Reconstruction Attitudes,” *Journal of Southern History*, 12 (1946): 469–86; Bernard A. Weisberger, “The Dark and Bloody Ground of Reconstruction Historiography,” *ibid.*, 25 (1959): 427–47; Vernon L. Wharton, “Reconstruction,” in Arthur S. Link and Rembert W. Patrick, eds., *Writing Southern History: Essays in Historiography in Honor of Fletcher M. Green* (Baton Rouge, 1965), 295–315; and John Hope Franklin, “Reconstruction and the Negro,” in Harold M. Hyman, ed., *New Frontiers of the American Reconstruction* (Urbana, Ill., 1966), 59–76.

omission of any consideration of the first two years of Reconstruction in order to make a strong case against, for example, the Radicals. Perhaps such an approach has merit in a court of law or in some other forum, but as an approach to historical study its validity is open to the most serious question.

Perhaps an even more important explanation for the difficulty in getting a true picture of Reconstruction is that those who have worked in the field have been greatly influenced by the events and problems of the period in which they were writing. That first generation of students to study the postbellum years “scientifically” conducted its research and did its writing in an atmosphere that made the conclusions regarding Reconstruction foregone. Different conclusions were inconceivable.²⁷ Writing in 1905 Walter L. Fleming referred to James T. Rapier, a Negro member of the Alabama constitutional convention of 1867, as “Rapier of Canada.” He then quoted Rapier as saying that the manner in which “colored gentlemen and ladies were treated in America was beyond his comprehension.”²⁸

Born in Alabama in 1837, Rapier, like many of his white contemporaries, went North for an education. The difference was that instead of stopping in the northern part of the United States, as, for example, William L. Yancey did, Rapier went on to Canada. Rapier’s contemporaries did not regard him as a Canadian; and, if some were not precisely clear about where he was born (as was the *Alabama State Journal*, which referred to his birthplace as Montgomery rather than Florence), they did not misplace him altogether.²⁹ In 1905 Fleming made Rapier a Canadian because it suited his purposes to have a bold, aggressive, “impertinent” Negro in Alabama Reconstruction come from some non-Southern, contaminating environment like Canada. But it did not suit his purposes to call Yancey, who was a graduate of Williams College, a “Massachusetts Man.” Fleming described Yancey as, simply, the “leader of the States Rights men.”³⁰

Aside from his Columbia professors, Fleming’s assistance came largely from Alabamians: Thomas M. Owen of the Department of Archives and History, G. W. Duncan of Auburn, W. W. Screws of the *Montgomery Advertiser*, and John W. Du Bose, Yancey’s biographer and author of *Alabama’s Tragic Decade*.³¹ At the time that Fleming sought their advice regarding his Reconstruction story, these men were reaping the first fruits of disfranchisement, which had occurred in Alabama in 1901. Screws’s *Advertiser* had been a vigorous advocate of disfranchisement, while Du Bose’s *Yancey*, published a decade earlier, could well have been a campaign document to make permanent the redemption of Alabama from “Negro-carpenter-bagger-Scalawag rule.”³² It is inconceivable that such

²⁷ For a discussion of the impact of the scientific study of history on research and writing, see W. Stull Holt, “The Idea of Scientific History in America,” in his *Historical Scholarship in the United States and Other Essays* (Seattle, 1967), 15–28.

²⁸ Fleming, *Civil War and Reconstruction in Alabama*, 523. Fleming knew better, for in another place—deep in a footnote—he asserted that Rapier was from Lauderdale, “educated in Canada”; *ibid.*, 519n.

²⁹ Loren Schweninger, *James T. Rapier and Reconstruction* (Chicago, 1978), xvii, 15.

³⁰ Fleming, *Civil War and Reconstruction in Alabama*, 12. For an account of Yancey and other white Southerners in the North to secure an education, see John Hope Franklin, *A Southern Odyssey: Travelers in the Antebellum North* (Baton Rouge, 1976), 45–80.

³¹ Fleming, *Civil War and Reconstruction in Alabama*, viii–ix; and Du Bose, *Alabama’s Tragic Decade, 1865–1874* (Birmingham, Ala., 1940). Du Bose’s work is a collection of his newspaper articles published in 1912.

³² Du Bose, *The Life and Times of William Lowndes Yancey* (Birmingham, Ala., 1892), 407–22.

men would have assisted a young scholar who had any plans except to write an account of the Reconstruction era that would support their views. In any case they could not have been more pleased had they written Fleming's work for him.

But the "scientific" historians might well have been less pleased if they had not been caught up in the same pressures of the contemporary scene that beset Fleming. They, like Fleming, should have been able to see that some of the people that Fleming called "carpetbaggers" had lived in Alabama for years and were, therefore, entitled to at least as much presumption of assimilation in moving from some other state to Alabama decades before the war as the Irish were in moving from their native land to some community in the United States. Gustavus Horton, a Massachusetts "carpetbagger" and chairman of the constitutional convention's Committee on Education in 1867, was a cotton broker in Mobile and had lived there since 1835. Elisha Wolsey Peck, the convention's candidate for chief justice in 1867, moved to Alabama from New York in 1825. A few months' sojourn in Illinois in 1867 convinced Peck that the only real home he could ever want was Alabama. Charles Mayes Cabot, a member of the constitutional convention of 1865 as well as of the one of 1867, had come to Alabama from his native Vermont as a young man. He prospected in the West in 1849 but was back in Wetumka in the merchandising business by 1852.³³ Whether they had lived in Alabama for decades before the Civil War or had settled there after the war, these "carpetbaggers" were apparently not to be regarded as models for Northern investors or settlers in the early years of the twentieth century. Twentieth-century investors from the North were welcome provided they accepted the established arrangements in race relations and the like. Fleming served his Alabama friends well by ridiculing carpetbaggers, even if in the process he had to distort and misrepresent.

In his study of North Carolina Reconstruction published in 1914, Joseph G. de Roulhac Hamilton came as close as any of his fellow historians to reflecting the interests and concerns of his own time. After openly bewailing the enfranchisement of the freedmen, the sinister work of the "mongrel" convention and legislatures, and the abundance of corruption, Hamilton concluded that Reconstruction was a crime that is "to-day generally recognized by all who care to look the facts squarely in the face." But for Reconstruction, he insisted, "the State would to-day, so far as one can estimate human probabilities, be solidly Republican. This was clearly evident in 1865, when the attempted restoration of President Johnson put public affairs in the hands of former Whigs who then had no thought of joining in politics their old opponents, the Democrats." Hamilton argued that in his own time some men who regularly voted the Democratic ticket would not call themselves "Democrats." In an effort to appeal to a solid Negro vote, the Republicans had lost the opportunity to bring into their fold large numbers of former Whigs and some disaffected Democrats. In the long run the Republicans gained little, for the Negroes, who largely proved to

³³ Thomas McAdory Owen, *History of Alabama and Dictionary of Alabama Biography*, 4 vols. (Chicago, 1921), 2: 845-46, 4: 1335, 3: 278. For a discussion of the problem of defining carpetbaggers in Alabama, see Bond, *Negro Education in Alabama*, 65.

be "lacking in political capacity and knowledge, were driven, intimidated, bought, and sold, the playthings of politicians, until finally their so-called right to vote became the sore spot of the body politic."³⁴ In his account of Reconstruction, which placed the blame on the Republican-Negro coalition for destroying the two-party system in North Carolina, Hamilton gave a warning to his white contemporaries to steer clear of any connection with blacks whose votes could be bought and sold if the franchise were again extended to them.

And the matter was not only theoretical. In 1914, while Hamilton was writing about North Carolina Reconstruction, Negro Americans were challenging the several methods by which whites had disfranchised them, and Hamilton was sensitive to the implications of the challenge. He reminded his readers that, after the constitutional amendment of 1900 restricting the suffrage by an educational qualification and a "grandfather clause," the Democrats elected their state ticket. His eye was focused to a remarkable degree on the current political and social scene. "The negro has largely ceased to be a political question," he commented, "and there is in the State to-day as a consequence more political freedom than at any time since Reconstruction."³⁵ The lesson was painfully clear to him, as he hoped it would be to his readers: the successful resistance to the challenges that Negroes were making to undo the arrangements by which they had been disfranchised would remove any fears that whites might have of a repetition of the "crime" of Reconstruction. Segregation statutes, the white Democratic primary, discrimination in educational opportunities, and, if necessary, violence were additional assurances that there would be no return to Reconstruction.

UNFORTUNATELY, THE PERSISTENCE OF THE DISPUTE over what actually happened during Reconstruction and the use of Reconstruction fact and fiction to serve the needs of writers and their contemporaries have made getting at the truth about the so-called Tragic Era virtually impossible. Not only has this situation deprived the last three generations of an accurate assessment of the period but it has also unhappily strengthened the hand of those who argue that scientific history can be as subjective, as partisan, and as lacking in discrimination as any other kind of history. A century after the close of Reconstruction, we are utterly uninformed about numerous aspects of the period. Almost forty years ago Howard K. Beale, writing in the *American Historical Review*, called for a treatment of the Reconstruction era that would not be marred by bitter sectional feelings, personal vendettas, or racial animosities.³⁶ In the four decades since that piece was written, there have been some historians who have heeded Beale's call. It would, indeed, be quite remarkable if historians of today were not sensitive to some of the strictures Beale made against those who kept alive the hoary myths about Reconstruction and if scholars of today's generation did not attempt to

³⁴ Hamilton, *Reconstruction in North Carolina*, 663.

³⁵ *Ibid.*, 666-67.

³⁶ Beale, "On Rewriting Reconstruction History," 807-27.

look at the period without the restricting influences of sectional or racial bias. And yet, since the publication of Beale's piece, several major works have appeared that are aggressively hostile to any new view of Reconstruction.³⁷ Nor has Beale's call been heeded to the extent that it should have been.

If histories do indeed reflect the problems and concerns of their authors' own times, numerous major works on Reconstruction should have appeared in recent years. After all, since the close of World War II this nation has been caught up in a reassessment of the place of Negroes in American society, and some have even called this period the "Second Reconstruction."³⁸ Central to the reassessment has been a continuing discussion of the right of blacks to participate in the political process, to enjoy equal protection of the laws, and to be free of discrimination in education, employment, housing, and the like. Yet among the recent writing on Reconstruction few major works seek to synthesize and to generalize over the whole range of the freedmen's experience, to say nothing of the problem of Reconstruction as a whole. Only a limited number of monographic works deal with, for example, Reconstruction in the states, the regional experiences of freedmen, the freedmen confronting their new status, aspects of educational, religious, or institutional development, or phases of economic adjustment.

In recent years historians have focused much more on the period of slavery than on the period of freedom. Some historians have been most enthusiastic about the capacity of slaves to establish and maintain institutions while in bondage, to function effectively in an economic system as a kind of upwardly mobile group of junior partners, and to make the transition to freedom with a minimum of trauma.³⁹ One may wonder why, at this particular juncture in the nation's history, slavery has attracted so much interest and why, in all of the recent and current discussions of racial equality, Reconstruction has attracted so little. Not even the litigation of *Brown v. The Board of Education*, which touched off a full-dress discussion of one of the three Reconstruction Amendments a full year before the decision was handed down in 1954, stimulated any considerable production of Reconstruction scholarship.⁴⁰ Does this pattern suggest that historians have thought that the key to understanding the place of Afro-Americans in American life is to be found in the slave experience and not in the struggles for adjustment in the early years of freedom? Or does it merely mean that historians find the study of slavery more exotic or more tragic and therefore more at-

³⁷ See, for example, Coulter, *The South during Reconstruction*; and Bailey, *The American Pageant*, chap. 24.

³⁸ See C. Vann Woodward, "The Political Legacy of Reconstruction," in his *The Burden of Southern History* (New York, 1961), 107.

³⁹ For some of the works that deal with these themes, see John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972); Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974); Gutman, *The Black Family in Slavery and Freedom*; and David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), and *The Problem of Slavery in the Age of Revolution, 1770-1823* (Ithaca, N.Y., 1975).

⁴⁰ A few who were associated with counsel for the plaintiffs have published some of their work. See, for example, Alfred H. Kelly, "The Congressional Controversy over School Segregation, 1867-1875," *AHR*, 64 (1958-59): 537-63; and John Hope Franklin, "Jim Crow Goes to School: The Genesis of Legal Segregation in Southern Schools," *South Atlantic Quarterly*, 58 (1959): 225-35.

tractive than the later period of freedom? Whatever the reason, the result has been to leave the major thrust of the Reconstruction story not nearly far enough from where it was in 1929, when Claude Bowers published *The Tragic Era*.

That result is all the more unfortunate in view of what we already know and what is gradually and painfully becoming known about the period following the Civil War. With all of the exhortations by Howard Beale, Bernard Weisberger, and others about the need for more Reconstruction studies, the major works with a grand sweep and a bold interpretation have yet to be written. Recent works by Michael Perman and Leon F. Litwack, which provide a fresh view respectively of political problems in the entire South and of the emergence of the freedman throughout the South, are indications of what can and should be done in the field.⁴¹ And, even if the battle for revision is being won among the professionals writing the monographs (if not among the professionals writing the textbooks), it is important to make certain that the zeal for revision does not become a substitute for truth and accuracy and does not result in the production of works that are closer to political tracts than to histories.

Although it is not possible to speak with certainty about the extent to which the Reconstruction history written in our time reveals the urgent matters with which we are regularly concerned, we must take care not to permit those matters to influence or shape our view of an earlier period. That is what entrapped earlier generations of Reconstruction historians who used the period they studied to shape attitudes toward problems they confronted. As we look at the opportunities for new syntheses and new interpretations, we would do well to follow Thomas J. Pressly's admonition not to seek confirmation of our views of Reconstruction in the events of our own day.⁴² This caveat is not to deny the possibility of a usable past, for to do so would go against our heritage and cut ourselves off from human experience.⁴³ At the same time it proscribes the validity of reading into the past the experiences of the historian in order to shape the past as he or she wishes it to be shaped.

The desire of some historians to use the Reconstruction era to bolster their case in their own political arena or on some other ground important to their own well-being is a major reason for our not having a better general account of what actually occurred during Reconstruction. To illustrate this point, we are still without a satisfactory history of the role of the Republican Party in the South during Reconstruction. If we had such a history, we would, perhaps, modify our view of that party's role in the postbellum South. We already know, for example, that the factional fights within the party were quite divisive. The bitter fight between two factions of Republicans in South Carolina in 1872 is merely one case in point. On that occasion the nominating convention split in two and each faction proceeded to nominate its own slate of officers. Only the absence of any opposition party assured a Republican victory in the autumn

⁴¹ Perman, *Reunion without Compromise: The South and Reconstruction, 1865-1868* (Cambridge, 1973); and Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979).

⁴² Pressly, "Racial Attitudes, Scholarship, and Reconstruction: A Review Essay," *Journal of Southern History*, 32 (1966): 90.

⁴³ J. R. Pole, "The American Past: Is It Still Usable?" *Journal of American Studies*, 1 (1967): 70-72.

elections.⁴⁴ In some instances blacks and whites competed for the party's nomination to public office, thus indicating quite clearly the task facing a Negro Republican who aspired to public office.⁴⁵ That is the task that John R. Lynch faced when he ran for Congress in 1872 and defeated the white incumbent, L. W. Pearce, who was regarded even by Lynch as "a creditable and satisfactory representative."⁴⁶ And it was not out of the question for white Republicans to work for and vote for white Democrats in order to make certain that Negro Republican candidates for office would be defeated.⁴⁷ So little is known of the history of the Republican Party in the South because the presumption has generally been that Lincoln's party was, on its very face, hostile to Southern mores generally and anxious to have Negroes embarrass white Southerners. Indeed, had historians been inclined to examine with greater care the history of the Republican Party in the South, they would have discovered even more grist for the Democratic Party mill.

Thus, studying works on Reconstruction that have been written over the last century can provide a fairly clear notion of the problems confronting the periods in which the historians lived but not always as clear a picture of Reconstruction itself. The state of historical studies and the level of sophistication in the methods of research are much too advanced for us to be content with anything less than the high level of performance found in works on other periods of United States history. There is no reason why the facts of Reconstruction should be the subject of greater dispute than those arising out of Askia Muhammad's rule in Songhay or Cromwell's rule in Britain. But we are still doing the spade-work; we are still writing narrowly focused monographs on the history of Reconstruction. We need to know more about education than Henry L. Swint, Horace Mann Bond, and Robert Morris have told us.⁴⁸ Surely there is more to economic development than we can learn from the works by Irwin Unger, George R. Woolfolk, Robert P. Sharkey, and Carl Osthaus.⁴⁹ And race, looming large in the Reconstruction era, as is usually the case in other periods of American history, is so pervasive and so critical that the matter should not be left to Herbert G. Gutman, Howard Rabinowitz, John H. and La Wanda Cox, Thomas Holt, and a few others.⁵⁰

⁴⁴ Edward F. Sweat, "Francis L. Cardozo—Profile in Reconstruction Politics," *Journal of Negro History*, 46 (1961): 217–32. For examples of other intraparty conflicts, see Robert H. Woody, "Jonathan Jasper Wright, Associate Justice of the Supreme Court of South Carolina, 1870–77," *ibid.*, 18 (1933): 114–31; and Schweninger, *James T. Rapier and Reconstruction*, 75, 144.

⁴⁵ Schweninger, *James T. Rapier and Reconstruction*, 114.

⁴⁶ Franklin, *Reminiscences of an Active Life: The Autobiography of John Roy Lynch*, 101–02.

⁴⁷ Thomas B. Alexander, *Political Reconstruction in Tennessee* (Nashville, 1950), 204–05, 240–41; and Taylor, *Louisiana Reconstructed, 1863–1877*, 214.

⁴⁸ Swint, *Northern Teacher in the South* (Nashville, 1941); Bond, *Negro Education in Alabama*; and Morris, "Reading, 'Ritin', and Reconstruction" (Ph.D. dissertation, University of Chicago, 1976).

⁴⁹ Unger, *The Greenback Era: A Social and Political History of American Finance, 1865–1879* (Princeton, 1965); Woolfolk, *The Northern Merchants and Reconstruction, 1865–1880* (New York, 1958); Sharkey, *Money, Class, and Party: An Economic Study of Civil War and Reconstruction* (Baltimore, 1959); and Osthaus, *Freedmen, Philanthropy, and Fraud: A History of the Freedmen's Savings Bank*.

⁵⁰ Gutman, *The Black Family in Slavery and Freedom*; Rabinowitz, *Race Relations in the Urban South* (New York, 1978); Cox and Cox, *Politics, Principles, and Prejudice, 1865–1866: Dilemma of Reconstruction America* (New York, 1963); and Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, Ill., 1977).

RECENT SCHOLARSHIP ON THE RECONSTRUCTION ERA leaves the impression that we may be reaching the point, after a century of effort, where we can handle the problems inherent in writing about an internal struggle without losing ourselves in the fire and brimstone of the Civil War and its aftermath. Perhaps we have reached the point in coping with the problems about us when we no longer need to shape Reconstruction history to suit our current needs. If either or both of these considerations is true, we are fortunate, for each augurs well for the future of Reconstruction history. It would indeed be a happy day if we could view the era of Reconstruction without either attempting to use the events of that era to support some current policy or seeking analogies that are at best strained and provide little in the way of an understanding of that era or our own.

“Not since Reconstruction” is a phrase that is frequently seen and heard. Its principal purpose is to draw an analogy or a contrast. Since it usually neither defines Reconstruction nor makes clear whether it is a signpost of progress or retrogression, searching for some other way of relating that period to our own may be wise, if not necessary. In the search for the real meaning of Reconstruction, phrases like “not since Reconstruction” provide no clue to understanding the period. Worse still, they becloud the relationship between that day and this. To guard against the alluring pitfalls of such phrases and to assure ourselves and others that we are serious about the postbellum South, we would do well to cease using Reconstruction as a mirror of ourselves and begin studying it because it very much needs studying. In such a process Reconstruction will doubtless have much to teach all of us.

From the Land of Canaan to the Land of Guinea: The Strange Odyssey of the “Sons of Ham”

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[Noah] became drunk and lay uncovered in his tent. And Ham, the father of Canaan, saw the nakedness of his father and told his two brothers outside. Then Shem and Japheth took a garment, laid it upon both their shoulders, and walked backward and covered the nakedness of their father; their faces were turned away, and they did not see their father's nakedness. When Noah awoke from his wine and knew what his youngest son had done to him he said,

“Cursed be Canaan;

a slave of slaves shall he be to his brothers.”

He also said,

“Blessed by the Lord my God be Shem;

and let Canaan be his slave.

God enlarge Japheth,

and let him dwell in the tents of Shem;

and let Canaan be his slave.”¹

THIS STRANGE STORY HAS TROUBLED THEOLOGIANS for perhaps two thousand years. Since it is first found in the Old Testament, a common source of the Jewish, Christian, and Muslim traditions, moralists have understandably expected to find in it some illustration of ethics or justice. Yet the story offends our moral sensibilities at almost every point. In the first place, even when the extreme sensitivity that the ancient Hebrews are supposed to have had about nudity is taken into account, Noah's curse dwarfs Ham's offense. By any ordinary standard, ancient or modern, Noah overreacted. Second, the curse fell not upon the offender at all, but upon Canaan, a son of Ham, who, in terms of the story, is

I have benefited greatly from the discussions that have followed the presentation of earlier versions of this article to the California State Polytechnic University, Pomona, chapter of Phi Alpha Theta, to the Southern California History Guild, and to the 1977 National Endowment for the Humanities seminar, “Development of Institutions and Ideology in Colonial America,” held at Northwestern University. Also, because the subject covers many areas of specialization, I have requested and received readings from an unusually large number of colleagues. Especially important suggestions have been offered by Timothy H. Breen, Richard Sigwalt, George M. Fredrickson, Daniel Pipes, L. Carl Brown, William H. McNeill, James M. McPherson, Herbert Shapiro, Eric Foner, Otto Olsen, David B. Davis, George B. Tindall, Thomas Reece, Jerome Van Camp, Charles Verlinden, Bernard Lewis, Hans Ruyter, and Richard D. Weis; and I appreciate their comments.

¹ Gen 9:21–27 (Revised Standard Version). All succeeding biblical references are enclosed in parentheses in the text; they are from the RSV unless otherwise noted.

totally innocent. Over the centuries theologians have often held ideas about nudity and respect for parents similar to those of the ancient Hebrews. Yet they have consistently failed to find in Ham's behavior sufficient justification for Noah's curse upon Canaan.²

The story of Ham presents far fewer difficulties, however, if it is considered not as a moral tale but as a sacred myth. One of the characteristics of myths, including sacred ones, is their amorality. In these accounts, divine or semidivine beings often commit deeds that violate or transcend the moral restraints of the ordinary people who believe in the myth.³ An ethnographical study of northern Ghana has shown that, among the Tallensi people, the most elaborate myths are those that interpret situations of sharp social polarization, that interpret the most tension-producing areas of their society.⁴ If such a principle has wide application, it would suggest that the Curse of Noah, which was molded and elaborated for more than two thousand years, has functioned to interpret an area of continuing social tension. This inquiry explores historically this area of tension in an effort to determine why certain peoples have come to be regarded as heirs to Noah's curse and others as heirs to his blessing, destined to have their needs served by ethnic inferiors. By studying the shifting ethnic identifications of the "sons of Ham," by following their journey in myth from the land of Canaan to the land of Guinea, we can perhaps learn something about the historical pressures that shaped modern white racial attitudes.

MOST SCHOLARS BELIEVE that the story of Noah's curse, in its biblical form, is a product of the kingdom of David and Solomon and thus reflects the circumstances of the Hebrews in the tenth century B.C. The Hebrews believed themselves and several neighboring peoples to be "Semites"—that is, descendants of one of the heroes of the tale, "Shem" (Gen. 10:22 ff.), who, in the various linguistic forms of his name, is also called "Sem" and "Sam." About 1200 B.C., their nomadic ancestors from the eastern desert had invaded the "land of Canaan," an invasion that coincided with one from the west by the Sea People, or Philistines. By the tenth century the Canaanites, the presumed descendants of the individual upon whom the curse had fallen, had been conquered, and many of them had been annihilated or enslaved. Yet it had become apparent that the victorious Hebrews would have to share the land and the Canaanites with those

² For efforts of critical scholars to solve some of the riddles posed by the story, see *The Anchor Bible: Genesis*, introduction, translation, and notes by E. A. Speiser (Garden City, N.Y., 1964), s.v. "Noah and His Sons," 60–63; *Encyclopedia of Islam*, s.v. "Ham," by A. Vajda; "Nuh [Noah]," by Bernard Heller. For the possible sexual implications of the Hebrew word *'erwat* (nakedness) see F. W. Basset, "Noah's Nakedness and the Curse of Canaan: A Case of Incest?" *Vetus Testamentum*, 21 (1971): 232–39. Interpretations of this kind go back at least to the rabbinical literature of the fourth to the sixth centuries. See *b. Sanhedrin*, 70a (Isidore Epstein, ed., *The Babylonian Talmud*, 35 vols. [London, 1935–52], 28: 477); English-language editions are cited in parentheses following the standard rabbinical reference. For historical interpretations, see note 5, below.

³ *International Encyclopedia of the Social Sciences*, s.v. "Myth and Symbol," by Victor W. Turner, 577.

⁴ Meyer Fortes, *The Dynamics of Clanship among the Tallensi* (New York, 1945), 26. For the relationship between myth and social tension, also see B. Malinowski, *Myth in Primitive Psychology* (New York, 1926), 58–59; and E. R. Leach, *Political Systems of Highland Burma: A Study of Kachin Social Structure* (Cambridge, Mass., 1954), 277–78.

other conquerors, the Philistines, who thus become in myth the sons of Japheth, dwelling "in the tents of Shem."⁵ If the story, despite its incorporation in the Jewish and Christian canons, seems to lack what many later readers liked to think of as the essential elements of divine justice, it at least has the merit of explaining events of history, which often seem to lack the essential elements of divine justice. The tradition provided a militarily successful people with religious sanction. It also provides an example of how myth translates the complex events of history into simple personal terms.

The story seems to say that all mankind is descended from the household of Noah, the single family that survived the Flood, and that one branch of the family of man is fated to perpetual slavery. In its original form, however, Noah's curse seems to justify the enslavement of a people, but a people who were racially indistinguishable from their conquerors. Indeed, according to the biblical account, the enslavers and the enslaved were descended from brothers. As yet the tradition showed no color bias.⁶ The Hebrews of the first millennium B.C., like other ancient peoples, did not associate slavery with any particular race. But, if ancient slavery was racially nonspecific, it was by no means ethnically so. There were always some peoples considered more appropriate for enslavement than others.⁷ For the purpose of understanding the rise of modern Western racial prejudices, it is important to consider the historical process whereby a people acquires or loses a slavish reputation, whereby slavery acquires or loses an ethnic identification.

The history of ancient Israel brings some light to this process, because the slavish reputation of the conquered Canaanites was the result of the position they occupied within the newly created Hebrew kingdom. David, Solomon, and their successors ruled an ethnically stratified society in which the ethnic distribution of power was celebrated by the Hamitic myth. The practice of slavery was by no means new to the Israelites during the period of the monarchy, nor had their modest conquests created vast armies of slaves such as those that flowed into the late Roman Republic. Yet the increase in slaveholding was sufficient to influence their lives and their thinking in a number of ways. "Canaanite slavery," for example, became the most debased form of exploitation that they knew, transmitting to each successive generation the meaning of the conquest and underscoring a difference in status between persons descended from the conqueror and those descended from the vanquished.

⁵ Gerhard von Rad, *Genesis: A Commentary*, trans. John Marks (rev. ed., London, 1972), 134–35. For a summary of scholarly opinions on the dating of the story, see Claus Westermann, *Genesis*, vol. 1: *Genesis, 1–11* (Neukirchen-Vluyn, Germany, 1974), 636–57.

⁶ See Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, 1968), 18; and Ephraim Isaac, "Biblical and Rabbinic Understanding of the Curse of Noah," *Sidic: Journal of the Service international de documentation judéo-chrétienne*, 11 (1978): 25. For racial perceptions in the biblical and Greco-Roman worlds, see R. A. Bennett, Jr., "Africa and the Biblical Period," *Harvard Theological Review*, 64 (1971): 489–90; and Frank M. Snowden, Jr., *Blacks in Antiquity: Ethiopians in the Greco-Roman Experience* (Cambridge, Mass., 1970).

⁷ Eduard Meyer, *Die Sklaverei im Altertum* (Dresden, 1898), 10–11; Henri Levy-Bruhl, "Esquisse d'une théorie sociologique de l'esclavage à Rome," in *Slavery in Classical Antiquity: Views and Controversies*, ed. M. I. Finley (Cambridge, 1960), 152–55; and M. I. Finley, "Was Greek Civilisation Based on Slave Labour?" in *ibid.*, 60–61.

Furthermore, Canaanite slavery changed power relationships within the Israelite community, increasing the hierarchical character of society. Previously, the Israelites had had neither true kings nor state slavery, which involved the harshest use of captives. Ironically, state slavery not only further degraded the reputation of the vanquished but also enabled the kings to function with a certain despotic independence that would not have been possible for the earlier leaders, whose authority had been derived from ties of religion and kinship with their followers. Royal captives worked in state enterprises such as the copper mines and refineries of 'Arabah, which gave the king an income for which he did not have to answer to his subjects. This enabled him to hire foreign mercenary troops, making him less dependent upon the popular levy (1 Chron. 18:11; 2 Sam. 20:23).⁸ There were also the *netinim*, the temple slaves, descendants of war captives, "hewers of wood and drawers of water for the congregation" (Josh. 9:21–27; also see Num. 31:26–47). The *netinim* functioned to enhance the power and prestige of the state religion.

Had the reputation of the Canaanites depended upon state and temple slavery, it probably would not have survived the downfall of the monarchy and the destruction of the first Temple. But many more Canaanites served in Hebrew homes, and household slavery continued to be a feature of Jewish communities for more than two thousand years. Household slaves probably received considerably better treatment than could be expected in the royal slave gangs. Their treatment, furthermore, unlike that of either state or temple slaves, was governed by biblical injunctions. These regulations reflected the ethnic distribution of power established by the conquest of Canaan. A type of servitude existed even for Hebrews. Because of poverty, or because he had been convicted of certain crimes, a Hebrew might lose his freedom. But this did not make him an *eved Kanaani*, a Canaanite slave. The laws and customs of the conquerors protected even their least successful or most delinquent members. Legally, he could not be sold against his will to a non-Hebrew. He could not be required to do certain types of work that were considered degrading (Deut. 25:39–40). He had legal recourse if mistreated. He had the right to be freed at the end of a six-year term (Deut. 15:12–18). Though these servants were *eved Ivri*, sometimes translated as "Hebrew slaves," their actual status more closely resembled that of the indentured servants of the seventeenth-century English colonies. The real slaves held by Hebrew masters were the "Canaanite slaves," whose legal enslavement was compounded by ethnic debasement (Lev. 25:29–46).⁹

⁸ Nelson Gluck, "The Boundaries of Edom," *Hebrew Union College Annual*, 11 (1936): 148–49; Isaac Mendelsohn, *Slavery in the Ancient Near East: A Comparative Study of Slavery in Babylonia, Assyria, Syria, and Palestine from the Middle of the Third Millennium to the End of the First Millennium* (New York, 1949), 96; and Roland de Vaux, *Ancient Israel: Its Life and Institutions*, trans. John McHugh, (New York, 1961), 89, 95, 123, 129.

⁹ E. E. Urbach, "The Laws regarding Slavery as a Source for Social History of the Period of the Second Temple, the Mishnah, and Talmud," in Institute of Jewish Studies, University of London, *Papers of the Institute*, 1, ed. J. G. Weiss (Jerusalem, 1964): 26–27; de Vaux, *Ancient Israel*, 80–83; David Farbstein, *Das Recht der unfreien und der freien Arbeiter nach jüdisch-talmudischem Recht* (Bern, 1896), 9–11; Zadoc Kahn, *L'esclavage selon la Bible et le Talmud* (Paris, 1867), 198–99; Simon Rubin, "Ein Kapitel aus der Sklaverei im talmudischen und römischen Rechte," in S. Krauss, ed., *Festschrift für Adolf Schwartz zum siebenzigsten Geburtstag, 15 Juli 1916* (Berlin, 1917), 212; and Mendelsohn, *Slavery in the Ancient Near East*, 105–06, 152.

In most societies it is unnecessary to explain the use of a double standard in the treatment of people of one's own kind and members of some out-group. Outsiders are treated differently because everything about them seems different. Furthermore, they are often perceived as inferior because, since most societies are ethnocentric, outsiders are judged by the only standards that one knows, that is, by the appearance, the speech, the customs of one's own group; and one's eyes and ears tell how badly an outsider falls short of the mark. As long as captives are ethnically distinct from their captors, their discriminatory treatment calls for no further apology.

Yet, as has been characteristic of other slave systems, Hebrew slavery functioned in conflict with its own justifying ideology. On the one hand, because the Canaanites were different, they could be required to do demeaning work, and they could be held to lifelong and hereditary servitude. But, on the other, the daily operation of the slavery system, with intimate contact between slaves and owners, inevitably made the Canaanites less different, as they assimilated the religion, the customs, and speech habits of the dominant group. Inevitably, ties of religion, friendship, and kinship arose between bond and free. Slaveholders occasionally wrote wills freeing their slaves who were sometimes their kinsmen or fellow believers, thereby denying heritable servants to their legitimate children.¹⁰ By one means or another, a number of slaves in each generation ceased to be slaves.

Yet slavery did not disappear, nor did the marginal status of the conquered population disappear. Legalists continued to distinguish between "Canaanite" and "Hebrew" slaves, though these two groups shared the same religion and culture. Throughout the centuries of the monarchs, the descendants of the vanquished remained *gerim*, "sojourners" in the land of their birth. After the Babylonian Captivity they became "proselytes." Their name had changed but their marginal status had not.¹¹ For a conservative process was functioning within Hebrew and, later, Jewish society that inhibited the total assimilation of converts to Jewish religion and culture, a process that maintained ancient distinctions established by distant wars of conquest. To understand how this process functioned we must look beyond the experience of the Jews and take a broader view of slave-holding societies.

THE PROCESS WHEREBY SLAVERY, especially household slavery, becomes ameliorated finds expression on the plane of ideas as paternalism. As the name implies, "paternalism" is associated with the patriarch, the head of a large, hierarchical household. The introduction of slaves into such a household enormously increases the authority of the patriarch in a way that is analogous to the manner

¹⁰ J. Pedersen, *Israel, Its Life and Culture*, trans. Aslang Møller, 1 (London, 1926): 40-41; Jean Juster, *Les Juifs dans L'Empire romain, leur condition juridique, économique, et sociale*, 2 (New York, 1914): 81; Salo Wittmayer Baron, *A Social and Religious History of the Jews*, 16 vols. (1937; 2d ed., rev. and enl., New York, 1952-), 1: 267-70; Urbach, "The Laws regarding Slavery as a Source for Social History," 48, *passim*; and David Farbstein, *Die Stellung der Juden zur Rassen- und Fremdenfrage* (Zurich, 1939), 21-22, 24, 27-28, 33.

¹¹ Pedersen, *Israel, Its Life and Culture*, 40-41; and de Vaux, *Ancient Israel*, 74-76.

in which the acquisition of state slaves by a king enhances his despotic independence. Because slaves lighten household chores, the “real” members of the family are likely to welcome the addition of these fictitious members up until that critical moment when the patriarch awards to a slave something that a free person wants for himself: a particularly competent slave is given a responsibility that a son covets; an adored bastard stands closer to the power of the patriarch than a legitimate child; or a young slave sleeps in a bed that a legal wife thinks is her rightful domain. In brief, the patriarch has created what Marc Henri Piault has called the “scandalous paradox”: a person who possesses neither legal rights nor social status has been given preference over one who possesses both.¹² In creating such tensions, a patriarch may merely be indulging his preferences. But, if he chooses, he can always favor a servile over a free member of the household, and this option serves to bring legitimate members of his family under tighter control.

Invariably, the free but dependent members of the patriarchal family, who may be called “legitimists,” react to the scandalous paradox. Sometimes the legitimist backlash comes with fearful violence, and the recipients of paternalistic favoritism discover the fragility of such benevolence. In Hebrew tradition the slave woman, Hagar, having become pregnant by Abraham, suffered harsh treatment at the hands of the patriarch’s legitimate wife and fled into the wilderness. Abraham did not lift a finger to help her (Gen. 16:1–6). In a more modern slave society there are reports of young concubines having their eyes gouged out by the jealous wives of plantation owners.¹³ Status anxieties, unleashed by the scandalous paradox, may trigger pogroms, lynchings, and race riots. But there is a less dramatic, yet highly important, way that legitimists defend their privileges: it is primarily they who perpetuate tradition, who educate children, who shape status-sustaining ideology. As individuals they may not be as powerful as the patriarch, particularly during his years of greatest vigor. But they outnumber him. The household does not function well without their cooperation. Their ideas tend to be regarded as the more respectable ones of slave-holding societies, while those of the paternalist, if the ideas go beyond empty words, are looked on as eccentric if not subversive.¹⁴ So to any status confusion that the patriarch in his senility may be generating, they reply with the clear-cut claims of the legitimate family. The pretensions of all others are the consequence of sin and abomination. In brief, slave-holding societies are character-

¹² Piault, “Captifs du pouvoir et pouvoir des captifs,” in Claude Meillassoux, *L’esclavage en Afrique précoloniale* (Paris, 1975), 333, *passim*.

¹³ Gilberto Freyre, *The Masters and the Slaves: A Study in the Development of Brazilian Civilization*, trans. Samuel Putnam (2d Eng. ed., New York, 1956), 351. Also see Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 333. In slave and former slave societies, furthermore, the traditionally free population, especially highly insecure groups such as immigrants, finding themselves in a position analogous to free but dependent persons in a servile household, have often embraced the myths and cruelties of legitimacy with all the passion of neophytes.

¹⁴ Thus in 1776, when Henry Laurens of South Carolina considered freeing some of his slaves, he anticipated that he would encounter the opposition of his children and that their views would be sustained by the community. Robert Carter of Virginia did, indeed, arouse the opposition of his sons and neighbors in 1791 by freeing five hundred slaves. See Clement Eaton, *A History of the Old South: The Emergence of a Reluctant Nation* (New York, 1975), 368, 370.

ized by a tension between two *elite*-class, ideological tendencies: paternalism, associated with the patriarch, which functions toward the elimination of slaves as a distinct status group within the hierarchy of the patriarchal family; and legitimacy, associated with the free, dependent members of the household, which incubates traditions, myths, and sexual taboos serving to protect old distinctions of status and privilege.

THIS TENSION BETWEEN PATERNALISM AND LEGITIMACY may explain why Jewish society, despite its strong paternalistic impulse, for centuries held at a distance the *gerim* and proselytes: marginal status groups that shared its faith and culture. In order for free-born but dependent legitimists to maintain a sharp distinction between themselves and such socially marginal groups as slaves, they either had to cultivate some new defensive ideology, as the original ethnocentric justification was reduced to fiction, as Canaanites became assimilated to the “chosen people,” or find new sources of slaves to replace those assimilated and emancipated. Both of these circumstances were present in the Hebrew kingdoms of the first millenium B.C. and in the later Jewish communities of the ancient and medieval Near East.

As to the first of these circumstances, the Hebrew legitimists, if they were to defend the line of social stratification defining their influence and privileges, had to cultivate some distance-creating idea that denied the legitimacy of the social ties that inevitably arose between bond and free. In the ancient and medieval Near East the necessary distance was created by the Hamitic myth, which always made possible a shift in the interpretation of the lowly status of the Canaanites from grounds of ethnic distance to grounds of birth. As Canaanites increasingly adopted Hebrew religion and customs and as their degradation became more difficult to explain by the inferiority of their culture, it could still be explained by Noah’s curse on Canaan—that is, by the inferiority of their birth. Differences in ancestry, however, are not always as obvious to the eye and ear as those of language and custom. People making castelike distinctions, therefore, often appeal to a justifying myth. American scholars have sometimes thought that there is no way as effective in marking a status frontier as a cluster of ideas associated with skin color. Yet a myth also serves fairly well: two thousand years after the conquest of Canaan, the Jewish philosopher Maimonides, in compiling a slave code, was still observing a distinction between “Canaanite” and “Hebrew” slaves.¹⁵

Undoubtedly, the general acceptance of this Curse of Noah slowed down the process of assimilation and emancipation. Nevertheless, even as early as the beginning of the Christian era, there were no longer enough bona fide Sons of Ham available to meet the demand for slaves. Since it proved to be easier to stretch the definition of “Canaanite” than it was to find new sources of slaves

¹⁵ Maimonides, *Mishnah Torah*, Book Twelve: Acquisitions, Treatise V: Laws Concerning Slaves (Isaac Klein, trans., *The Code of Maimonides*, Yale Judaica Series, ed. Julian Obermann, 20 vols. [New Haven, 1951], 5: 245–81).

locally, the custom arose of importing “Canaanites” into the ancient land of Canaan. During the first Christian centuries most “Canaanites” were in fact either Syrians or *Kushim* from black Africa.¹⁶ In the early Middle Ages the concept was stretched even further. In the ninth and tenth centuries, for example, as a result of intertribal wars in the area of modern Yugoslavia, thousands of captives from that region flooded the medieval slave markets. Some of these were sold as “Canaanites” in the Jewish communities of the Near East.¹⁷ The subtlety of the theologian and the craft of the lawyer could transform blue-eyed Slavs and black-skinned *Kushim* alike into fellow descendants of Canaan, both laboring under the same devastating curse, the validity of which had grown no weaker over the centuries than had the demand for slaves.

The “sons of Ham” had thus begun their journey that would lead them along the tedious and twisted paths of Jewish, Muslim, and Christian theology and jurisprudence and would bring them one day to the shores of Guinea and beyond. But, before considering the historical pressures that darkened their faces and changed the way their hair grew, the problem of the justification of slavery in Europe needs to be examined. After all, slavery played a less prominent part in the life of the early Jews than it did in the Greco-Roman lands to the west.

MORE WIDELY ACCEPTED than the Hamitic myth during antiquity was a “sambo” perception of slaves. Roman, Greek, and Hebrew masters all referred to the fully adult men and women they owned by the linguistical equivalents of “boy” and “girl.” European slaveholders, moreover, would have found little to disagree with in the views of ancient Talmudists, who believed that slaves had a predilection for lying, laziness, theft, drunkenness, and lewdness.¹⁸ Furthermore, the Europeans, like the peoples of the Near East, engaged in the stereotyping of slaves. But beyond the “sambo” perception, which all shared, the stereotypes that emerged in the two regions were different. In Europe for a time, Scythians were regarded as a slavish people. Then Thrace became an important source of supply for the slave trade and Thracian characteristics, such as red hair, came to be regarded as slave characteristics. An actor on the Roman stage wore a red wig as a signal to the audience that he was playing the role of a slave. The Thra-

¹⁶ J. Klausner, “The Economy of Judea in the Period of the Second Temple,” in Michael Avi-yonah *et al.*, eds., *The Herodian Period*, vol. 7 of *The World History of the Jewish People*, ed. Cecil Roth (New Brunswick, N.J., 1975), 194; and Baron, *Social and Religious History of the Jews*, 2: 259–60.

¹⁷ Cecil Roth, “Economic Life and Population Movements,” in Cecil Roth, ed., *The Dark Ages: The Jews in Christian Europe, 711–1096*, vol. 11 of *World History of the Jewish People*, 27–28; Ivan Hrbek, “Die Sklaven im Dienste der Fatimiden,” *Archiv Orientali*, 21 (1953): 549–50; and Johannes Hoffmann, “Die östliche Adriaküste als Hauptnachschiebbasis für den venezianischen Sklavenhandel bis zum Ausgang des elften Jahrhunderts,” *Vierteljahresschrift für Sozial- und Wirtschaftsgeschichte*, 55 (1968): 172, 180.

¹⁸ Baron, *Social and Religious History of the Jews*, 1: 269–70; and Henri Wallon, *Histoire de l’esclavage dans l’antiquité*, 2 (Paris, 1847): 8. For the same tradition among the Arabs, see Adam Mez, *The Renaissance of Islam*, trans. S. H. Bakhsh (Patna, India, 1967), 162. “Sambo,” the Hausa word for “second son,” in American usage has come to designate a stereotype of the American black, who has been depicted as “docile but irresponsible, loyal but lazy, humble but chronically given to lying and stealing; his behavior was full of infantile silliness and his talk inflated with childish exaggeration”; Stanley M. Elkins, *Slavery: A Problem in American Institutional Life* (3d ed., Chicago, 1976), 82. This work is a much-disputed interpretation of the “sambo” stereotype.

cian name, "Rufus," came to be regarded as a typical slave name regardless of the slave's nationality.¹⁹

In response to patterns of political disorder or military conquest, however, the principal source of European slaves kept shifting, and early ethnic stereotypes lost all credibility. Various Western European nationalities at times became important sources of slaves, although not for a long enough period for any of them to acquire a slavish reputation. Gaul, for example, was an important source immediately after the Roman conquest in the first century B.C. And the political and ethnic fragmentation of Britain, following the collapse of Roman power, briefly created a surplus of slaves for export in that country.²⁰ In the later Middle Ages, as stable governments emerged in the West, the source of European slaves became confined increasingly to the lands around the Black Sea, areas that continued to feed the Mediterranean slave trade from remote antiquity down to the beginning of modern times. Because every major ethnic stereotype of European slaves has been drawn from this region, a glance at its history may shed some light on how certain peoples acquire a slavish reputation.

Since prehistoric times the rich black soil country north and west of the Black Sea has attracted settlers. But this territory has been the scene of a repetitive disaster. Periodically, nomadic invaders from Central Asia have overrun the western steppes, annihilating the inhabitants or selling them into slavery. Ironically, those escaping the invaders and taking refuge in the Caucasus Mountains to the east of the Black Sea helped create still another source for the slave trade. The remnants of many peoples have intruded into the territory of earlier refugees where they competed for the resources of a region in which much land is non-productive or marginal. Modern Caucasia is one of the most ethnically complex regions of the world. More than fifty languages are spoken in an area slightly larger than California.²¹ In ancient times the ethnic fragmentation may have been even greater. Strabo reported that seventy languages were used in a trading town located near modern Sukhumi, and Pliny the Elder says the Romans in the same area used one hundred and thirty interpreters "for the purpose of transacting business."²² From antiquity until modern times the poverty and the ethnic conflict of the region delivered a continuing stream of Caucasians to the

¹⁹ M. L. Gordon, "The Nationality of Slaves under the Early Roman Empire," in Finley, *Slavery in Classical Antiquity*, 174.

²⁰ But a measure of reunification took place within a hundred years, although some young Englishmen were still being shipped out of Bristol as late as the eleventh century. See Marc Bloch, "Comment et pourquoi finit l'esclavage antique," in Finley, *Slavery in Classical Antiquity*, 224-26; and Charles Verlinden, *L'esclavage dans l'Europe médiévale*, vol. 1: *Péninsule Ibérique-France* (Brugge, 1955), vol. 2: *Italie, colonies italiennes du Levant, Levant latin, Empire byzantin* (Ghent, 1977), 1: 701.

²¹ Moses I. Finley, "The Black Sea and the Danubian Regions and the Slave Trade in Antiquity," *Klio: Beiträge zur alten Geschichte*, 40 (1962): 51-55; *Great Soviet Encyclopedia*, translation of the 1973 edition, s.v. [Caucasus] "Historical Survey" by S. A. Zalesskii; Bernard Geiger *et al.*, *Peoples and Languages of the Caucasus: A Synopsis* (The Hague, 1959); and William H. McNeill, *Europe's Steppe Frontier, 1500-1800* (Chicago, 1964).

²² Strabo, *Geography*, 11: 2, 16; and Pliny, *Natural History*, 6: 5. Some relationship appears to exist between this ethnic diversity and the political fragmentation that has characterized Caucasia and other slave-exporting regions. During the eleventh century, for example, in the one-hundred-and-ninety-mile stretch between Lake Van and the Black Sea coast, there are reported to have been some dozen rival "kingdoms." W. E. D. Allen, *A History of the Georgian People from the Beginning down to the Russian Conquest in the Nineteenth Century* (London, 1932), 85.

slave traders. During the later Middle Ages certain Caucasian peoples such as the Circassians, the Abkhaz, and the Mingrelians were scarcely known outside of their native region except as slaves. Ibn Botlan, an eleventh-century Arabic-speaking Christian physician of Baghdad, described another Caucasian people in distinctly slavish terms:

The Armenian is the worst of the white, as the negro is of the black. . . . Chastity is unknown and theft is rampant among them. But they know not avarice. Coarse is their nature and coarse is their speech. Let an Armenian slave be an hour without work and he will get into mischief. He only works under the threat of the cane or the stress of fear. When you find him lazy—it is simply because he delights in laziness and not because he does not feel equal to work. You must then take to the cane, chastise him and make him do what you want.²³

Long debased by powerlessness, poverty, and slavery, Caucasian peoples have also been burdened with a “sambo”-like reputation that persisted a long time indeed. Even in the late nineteenth century an influential encyclopedia could write of a group of nationalities who lived in what is now the Soviet Republic of Georgia that “the one trait of these generally very poor populations is their care-free attitude. Living from day to day, these peoples think only of adorning themselves in brightly colored clothes that bring out their physical good looks. Singing and dancing are their favorite pleasures. Despite their mediocre intelligence these Caucasians know how to capture the sympathy of foreigners by their grand hospitality and gay friendly manner.”²⁴

During the late Middle Ages, however, the most prominent stereotypes of European slaves were not derived from the steady trickle of humanity flowing from Caucasian lands but from the larger disasters and more numerous peoples to the north and west of the Caucasians. In the late fourteenth century, for example, when Tamerlane’s armies overran the Golden Horde, the slave markets of Italy were flooded with Tartars—a situation that created for a time a Tartar stereotype for slaves in that country.²⁵ The Slavic peoples of the area suffered a similar fate. Indeed, far more important than the brief Tartar stereotype was the Slavic one, which predominated during the latter half of the Middle Ages in many countries. The Slavs inhabited, in addition to the western steppes, other areas that made them vulnerable. Many lived on the Balkan peninsula, which, like Caucasia, was an ethnically and politically fragmented mountain refuge for defeated peoples of the steppes. For centuries Balkan Slavs were victims of the Venetian slave traders. Also victims were the Central European Slavs, who lived in the path of the German medieval eastward expansion, the *Drang nach Osten*. In

²³ Ibn Botlan, “Introduction to the Art of Making Good Purchases of Slaves,” as quoted in Mez, *The Renaissance of Islam*, 162. In certain contexts, especially when comparing themselves to more northerly peoples, Arabs of this period thought of themselves as “black”; see Karl Vollers, “Über Rassenfarben in der arabischen Literatur,” in G. Pitrè et al., eds., *Centenario della nascita di Michele Amari* (Palermo, 1910), 87; and Wolfdietrich Fischer, *Farb- und Formbezeichnungen in der Sprache der altarabischen Dichtung* (Wiesbaden, 1965), 338.

²⁴ *La Grande Encyclopédie* [1885–92 ed.], s.v. “Caucase,” by P. Lemosof.

²⁵ Domenico Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV* (Genoa, 1971), 13–14; and *Great Soviet Encyclopedia*, s.v. “Golden Horde,” by V. I. Buganov.

virtually every language in Western Europe, the word for slave derives from this politically fragmented and perpetually vulnerable people.²⁶

WHILE SHIFTING ETHNIC STEREOTYPES FOR SLAVES emerged in ancient and medieval Europe, a relatively stable, *racial* stereotype for slaves emerged at the same time in the Near East. Just as most European slaves, and all of the major stereotypes of them, were drawn from the perpetually vulnerable peoples around the Black Sea, so the peoples of black Africa played a corresponding role in the slave trade of the Near East. As Near Eastern slaves more and more came from below the Sahara, the people of the Near East increasingly equated "slaves" with "Negroes." Crucial though this development is for understanding the evolution of the Hamitic myth and the origins of modern racial prejudice in general, the various stages of the process cannot be dated with absolute certainty. Its broad outlines, however, are known. The New Testament contains no hint of any identification of "slave" with "Negro." To be sure, these books from the later first and early second centuries are in Hellenistic Greek and leave the imprint of Hellenism on certain traditions of the Semitic-speaking peoples. In the Babylonian Talmud and certain Jewish midrashim there is a suggestion, albeit tenuous, of such identification. These works, which scholars have ascribed to the fourth through the sixth centuries A.D. (or C.E.), seem to betray little European influence. The equation of "slave" with "Negro" is missing, however, from the Quran (Koran), a highly indigenous Arabian work of the early seventh century. Only from the eighth century on, in several types of Muslim literature, does the fusion of the notions of "black" and "debasement" appear unmistakably and continually.

The differences between European and Near Eastern stereotypes for slaves resulted from the differing nature of the slave trade in the two regions. In Europe, until very late in the Middle Ages, not only the African slave trade but also all direct trade with sub-Saharan Africa was of minor importance. The world's greatest desert posed a formidable barrier between the two regions. Not until the second half of the fifteenth century was the Sahara bypassed on the west and relatively easy maritime contact established between Europe and black Africa. But some three thousand years earlier the Sahara was bypassed on the east. Even as early as the Middle Kingdom in the second millennium B.C., the Egyptians constructed a Nile-Red Sea canal and established Red Sea ports serving

²⁶ Meyer, *Die Sklaverei im Altertum*, 11; Hrbek, "Sklaven im Dienste der Fatimiden," 547, 552; Hoffmann, "Die östliche Adriaküste als Hauptnachschubbasis für den venezianischen Sklavenhandel," 170, 172, 179, *passim*; and Antonio Teja, "Aspetti della vita economica di Zara dal 1289 al 1409," pt. 2: "La schiavitù domestica ed il traffico degli schiavi," *La Rivista Dalmatica*, 22 (1941): 29-30. For the derivation of the word slave in various languages, see Verlinden, *L'Esclavage dans l'Europe médiévale*, 2: 999-1010. Another study yields still further evidence for the slavish reputation of medieval Slavs. The Arabic term for "eunuch" is derived from the ethnic root *Sakhab* (Slav), while the Catalan term *esclavó* (castrated goat) and the Spanish *eslabón* (link of chain) are likewise taken from the name of East European captives who were sometimes castrated or chained. See Henry Kahane and Renée Kahane, "Notes on the Linguistic History of Sclavus," in *Studi in onore di Ettore Lo Gatto e Giovanni Maver* (Florence, 1962), 346, 350-51, 357.

their trade with black Africa. Early in the first millennium B.C., the first Hebrew kingdom also had such a port (1 Kings 9:26–28). Furthermore, the Nile valley, fertile and densely settled, led like a road from the Mediterranean to black Africa.²⁷

From remote antiquity the Near East imported slaves from black Africa. Egyptian paintings depict Negro captives. For a very long time this trade was relatively small though growing. Probably as late as the seventh century A.D. more local natives than blacks were sold as slaves in the Near East. Before the establishment of Islam, this ancient battleground of nations, itself, supplied considerable numbers of captives to the slave trade. It may have been as late as A.D. 1000 before the institution of slavery in the Near East and Muslim lands became predominantly Negro slavery.²⁸ But long before a majority of slaves were black, perhaps several centuries before, enough Africans were sold in the Near East to form the basis for a racial stereotype for the region comparable to the Thracian or the later Slavic stereotypes farther west. While still a minority of the servile population, blacks could be recognized as members of the lowest status group, foreign slaves, those sold beyond reach of kin and hope of ransom. This bottom stratum of the society, which included only a small minority of the white population, accounted for the overwhelming majority of the blacks. It was they who became the instantly visible “Canaanites,” the archetypal “sons of Ham.”²⁹

More than ten centuries separate the appearance of the story of Ham in the book of Genesis from the elaboration and explanation of the tale that occurs in rabbinic literature. During these centuries the face of servitude had darkened in the Near East. In retelling the story, one rabbi correspondingly darkened the face of Ham and made the curse of Noah read, “Your seed will be ugly and dark-skinned.” If this writer magnified the curse, another likewise increased the offense for which it was a punishment: “Ham and the dog copulated in the Ark, therefore Ham came forth black-skinned, while the dog publicly exposes its copulation.”³⁰ Still another commentator further vilified the delinquent ancestor of slaves by suggesting that Ham had not only looked upon his father’s nakedness

²⁷ José Antonio Saco, *Historia de la esclavitud desde los tiempos mas remotos hasta nuestros días*, 1 (2d ed., Havana, 1936): 23–25; and Levi Herzfeld, *Handelsgeschichte der Juden des Altertums* (Braunschweig, 1879), 128, 221–22, 233. For a discussion of caravan trade and the Sahara as a barrier, see E. W. Bovill, *The Golden Trade of the Moors* (New York, 1958). For the navigational problems that delayed the development of a sea route around the western Sahara, see Raymond Mauny, *Les navigations médiévales sur les côtes sahariennes antérieures à la découverte portugaise* (Lisbon, 1960). For evidence of a black slave trade in Pharaonic times, see Wallon, *Histoire de l’esclavage dans l’antiquité*, 1: 27; for the trade in the first century A.D., Raymond Mauny, trans., *Périple de la mer Erythrée* (Paris, 1968), 25–26; and, for the early Islamic period, Y. F. Husan, *The Arabs and the Sudan from the Seventh to the Early Sixteenth Century* (Edinburgh, 1967), 42–50.

²⁸ That slaves were predominantly black in Egypt during the period ca. 950–1250 is evident from the Geniza documents. See S. D. Goiten, *A Mediterranean Society*, vol. 1: *Economic Foundations* (Berkeley and Los Angeles, 1967), 130–47. Also see Mez, *The Renaissance of Islam*, 157–58; and Gernot Rotter, “Die Stellung des Negers in der islamisch-arabischen Gesellschaft bis XVI Jahrhundert” (Ph.D. dissertation, Rheinische Friedrich-Wilhelms-Universität, Bonn, 1967), 52. Rotter’s fine doctoral dissertation has been my chief authority on Muslim race relations and racial attitudes.

²⁹ Bernard Lewis, *Race and Color in Islam* (New York, 1971), 27–28.

³⁰ *Bereshith Rabbah*, 36. 7 (H. Freedman and Maurice Simon, trans. and eds. *Midrash Rabbah*, 10 vols. [London, 1959], 1: 293). Also see *b. Sanhedrin*, 108b (Epstein, *Babylonian Talmud*, 20: 745).

but had castrated him as well.³¹ Clearly, a person who believed these words and accepted the biblical tradition of hereditary sin had to conclude that the treatment slaves were getting was about what they deserved. And, indeed, such conclusions, along with fragments of the rabbinic commentary, have been echoed down over the centuries, from Babylon to New Orleans, often by men who have been generally innocent of any other bits of Hebrew learning.

The racial stereotyping of slaves by Jews does not appear to have been elaborated beyond the rabbinic expressions of the period from the fourth to the sixth century. During the Middle Ages the Jews became to a larger extent a European as well as a Near Eastern people, and they came to share the stereotypes of both regions. Blacks continued to be "Canaanites"; but at the same time, by some logic best understood by theologians and lawyers, the Slavonic language, ancestor of the modern Slavic languages, became the "language of Canaan."³² After the sixth century, it was those less Europeanized sons of Shem, the Arabs, who further developed the tradition of Ham.

SIXTH-CENTURY, PRE-ISLAMIC ARABS may have already begun to equate blacks with slavery and debasement, although the evidence is questionable.³³ Such an equation is not to be found in the Quran or in the early Islamic movement of the next century. All races were welcomed into the faith, and the Quran lashes out against pride in family lineage, adding that the "most honoured . . . in the sight of God is he who is the most righteous." And the Quran views the linguistic and racial differences of man without any Arab ethnocentrism but sees them as a manifestation of a divine miracle.³⁴ Furthermore, in Muslim society, as in Jewish society, slavery never became the predominant labor system. The Quran possibly reaffirms the Semitic tradition concerning the descendants of Ham. It is free, however, of the color prejudice that seems to have influenced slightly earlier rabbinic versions of the tradition; and there is no condemnation to slavery. Rather than a curse, it contains a vaguely directed threat coming not from Noah but from God himself: when the waters of the Great Flood had subsided, God commanded Noah to "come down from the Ark with peace from us, and blessings on thee and on *some* of the peoples who will spring from those with thee: but there will be other peoples to whom we shall grant their pleasures for a

³¹ *Wayyikra Rabbah*, 17.5 (Freedman and Simon, *Midrash Rabbah*, 4: 219).

³² A twelfth-century Jewish scholar, arriving in Prague from Germany, wrote, "Here begins Scлавonia, called by the Jews who inhabit it *Kh'na'an*, because the inhabitants sell their children to all nations, which is also applicable to Russia"; *The Itinerary of Rabbi Benjamin of Tudela*, trans. A. Asher (New York, n.d.), 164. For Slavonic as the "language of Canaan," see Jacob Winter, *Die Stellung der Sklaven bei den Juden in rechtlicher und gesellschaftlicher Beziehung* (Halle, 1886), 5-6, n. 5.

³³ One Islamic scholar has given some weight to this evidence; see Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 25, 75-78. Another has held that antiblack statements attributed to sixth-century poets "almost certainly belong to later periods and reflect later problems . . ."; see Lewis, *Race and Color in Islam*, 9.

³⁴ Quran, 49: 13, 30: 22 (Abdullah Yusuf Ali trans. [Mecca, 1956]). But, since Arabic is the sacred language of Islam, an element of ethnocentrism was inescapable.

time, but in the end will a grievous penalty reach them from us.”³⁵ In years to come Muslim slaveholders would make considerable use of the Hamitic myth, but the Quranic version would not prove to be their most effective weapon.

In view of the broad-minded ethnic and social attitudes of the Prophet as well as the noblest of his followers, it is ironic that the lands of Islam became the cradle of modern racial stratification and of many of the ideas that are still used to justify special privileges defined by skin color and other racial characteristics. Muslims aspired to a universal brotherhood of believers. But prominent among their actual achievements was the forging of new links between blackness and debasement. It was under the Muslims that slavery became largely a racial institution.

An important reason why Muslim slavery became Negro slavery was because the rise of Islam eliminated from the Mediterranean slave trade an important source of light-skinned slaves. Previously, the military and political disorder of the Near East had filled the slave markets with captives; as long as these conditions persisted, most slaves remained racially indistinguishable from their masters.³⁶ But the Muslim ideal of brotherhood united the Bedouins, redirected their fierce fighting qualities outward, and assimilated their energies into a dramatic campaign of conquest. When the frontiers of Islam became comparatively stabilized, one hundred years after the death of the Prophet, the movement had established Muslim hegemony over a vast area extending from the Iberian peninsula to the borders of China. The political reality of unity found an idealized expression in the Sunna, or Islamic law, which held that no freeborn Muslim could be sold into slavery. Nor could a “dhimmi”—that is, a person who remained a Jew or a Christian but who lived under the protection of a Muslim government—be legally enslaved.³⁷ If the highly assimilationist Muslim system of slavery was to endure, new sources of captives had to be found outside of the *Dar al-Islam*, the countries living under the peace of Islam.

Lawful captives had to be taken either north of the Mediterranean or south of the Sahara; and, indeed, until well into the modern period, Muslims acquired slaves from each of these regions, though in highly unequal numbers. During the later Middle Ages a number of European states developed, with sophisticated military organizations that could answer the challenge of Islam blow for blow. European rulers, furthermore, could often recover their captured subjects by prisoner exchange or ransom.³⁸ But, basically, few European slaves were available for purchase during the later Middle Ages because of the more orderly political conditions in France, England, the German Empire, and other countries. As these states ceased producing a surplus of slaves for export, the supply

³⁵ Quran, 11: 48; italics added. But, since slaves did not have “their pleasures for a time,” the threat may not be directed against the sons of Ham, as is sometimes supposed.

³⁶ Verlinden, *L’esclavage dans l’Europe médiévale*, 1: 228.

³⁷ Laura Veccia Vaglieri, “The Patriarchal and Umayyad Caliphates,” in P. M. Holt *et al.*, eds., *The Cambridge History of Islam*, 1: 57–103; and *Encyclopaedia of Islam*, new ed., s.v. “Dhimma,” by Claude Cahen.

³⁸ Verlinden, *L’esclavage dans l’Europe médiévale*, 1: 541–43; and Iris Origo, “The Domestic Enemy: The Eastern Slaves in Tuscany in the Fourteenth and Fifteenth Centuries,” *Speculum*, 30 (1955): 328. Also see Hans Müller, “Sklaven,” in B. Spuler, ed., *Handbuch der Orientalistik*, pt. 1: *Der Nahe und der Mittlere Osten*, 6, sec. 6 (Leiden, 1977): 68–72.

from Europe was reduced to a minimum, and those came mainly from the Slavic lands.³⁹ Both in Europe and in black Africa the slave trade was governed by similar considerations. But, after about the eleventh century, political conditions south of the Sahara were more favorable to slave traders. To be sure, there were strong states in black Africa, such as those of the western Sudan, that protected their inhabitants from slave raids; but most black Africans lived in ethnically fragmented, often mutually antagonistic, societies that could offer little resistance to raids from the Sudanese or other Muslim states. It is no accident, therefore, that in late medieval Arabic poetry and popular literature *Banu Ham*, the “sons of Ham,” is a synonym for *sudan*, the Negroes.⁴⁰

Not only did slavery become largely a racial institution in a broad belt of countries extending from Andalusia to the Indian Ocean, but a related development also took place in these countries that forged even tighter links between blackness and debasement, links that have endured into the modern era. As early as the ninth century, racial stratification began to appear in *both* the servile *and* the free populations.

A line of racial stratification dividing the servile population can be seen in the evolution of two Arabic words. *‘Abd* (plural *‘abīd*), the classical term for slave, continued to be used in its original sense in legal and other formal literature. But in popular literature the term was no longer applied to a white, regardless of legal status. A European slave was now most often referred to as a *mamluk* (owned). *Mamluks* were rarer—and, hence, more expensive—than the *‘abīd*.⁴¹ European slaves, furthermore, had special uses that also enhanced their value—in prisoner exchanges, for example, to recover family members captured by Christians in the religious wars or to obtain substantial ransoms from wealthy families for Christian knights. Even those of a lower class origin were a potential source of ransom; for Europe, unlike black Africa, had well-organized institutions, such as *El Orden de la Merced*, that specialized in raising money to ransom Christian captives.⁴² To be a white slave was to be a valuable slave, and in the history of servitude relative value, more than any other factor, influenced the treatment that an individual could expect.

³⁹ Mez, *The Renaissance of Islam*, 158. The Mediterranean slave trade was actually more complex than the above description suggests. Some slaves came from more distant lands such as Central Asia and even India. Also the Ottoman Turks in the fifteenth and sixteenth centuries brought about a vigorous revival of white slavery. But until 1453 the Christians controlled the Straits and, hence, access to the Black Sea source of slaves. See Albert Howe Lybyer, *The Government of the Ottoman Empire in the Time of Suleiman the Magnificent* (Cambridge, Mass., 1913), 45–61.

⁴⁰ Rotter, “Die Stellung des Negers in der islamisch-arabischen Gesellschaft,” 22, 141. *Sudan* as a geographical term is derived from *Bilad al-Sudan*, “the land of the blacks.” For the relationship between state power and the slave trade in this region, see Marian Malowist, “Le Commerce d’or et d’esclaves au Soudan occidental,” *Africana Bulletin*, 4 (1966): 61, *passim*.

⁴¹ *Encyclopaedia of Islam*, new ed., s.v. “‘Abd” by R. Brunschvig; and Elivahu Ashtor, *Histoire des prix et des salaires dans l’orient médiéval* (Paris, 1969), 58–59, 361–63, 437–38. In Baghdad in the ninth and tenth centuries a “good-looking, but untrained white slave-girl fetched 1,000 dinars or more,” while in Oman, near the mouth of the Persian Gulf, blacks were selling for the equivalent of 25–30 dinars; Mez, *The Renaissance of Islam*, 157–58. For the racial connotation that the word *‘abd* assumed, see Leon Carl Brown, “Color in North Africa,” in John Hope Franklin, ed., *Color and Race* (Boston, 1968), 193.

⁴² Other such institutions included the Order of the Trinitaries, Santa Maria della Mercede, and Riscatto Schiavi. See Verlinden, *L’esclavage dans l’Europe médiévale*, 1: 541–43; Origo, “Domestic Enemy,” 328, 330; and Gioffrè, *Mercato degli schiavi a Genova*, 51–52 n. 115.

The favored treatment of *mamluks* is especially important because of the direction that the institution of slavery took in Islamic countries beginning roughly in the eighth century. The legitimist impulse all but collapsed.⁴³ Muslim patriarchs were, therefore, inhibited by few legal restrictions or social taboos in their paternalistic management of slaves, a development that resulted in hundreds of thousands of people, made captives by the wars of conquest, passing completely through the institution in about three generations to become fully integrated into Islamic society. Thus, Muslim slavery had few parallels in the degree of its paternalism. The “scandalous paradox” was everywhere in evidence as sons of slave mothers became sultans who governed through slave administrators, whose courts were protected by elite guards, entertained by highly trained musicians and dancers, all of whom were slaves. Though their servile status was more than a legal fiction, these slave elites sometimes exercised considerable power and enjoyed a splendid life style.⁴⁴ Under the Fatimid caliphate of the tenth and eleventh centuries, for example, *mamluks* of mostly Turkic or Slavic origin administered Egypt and other countries of the Near East and North Africa.

Black slaves, or *‘abīd*, however, were less expensive and consequently more expendable. Tens of thousands of black Africans, for example, labored on land reclamation projects in Iraq. Blacks were also used in the copper and salt mines of the Sahara. Wherever the work was demanding and the conditions harsh, black slaves were likely to be found.⁴⁵ Furthermore, when slaves were emancipated, the old line of racial stratification that divided the servile population into *mamluks* and *‘abīd* continued to affect their status as freedmen. Because of the almost unparalleled paternalism of Muslim slavery, patriarchs showed a marked tendency to assimilate fresh captives to Islamic culture, convert them, and later emancipate them.⁴⁶ Although creating a demand for freshly captured pagans, this process also produced a continuous growth of the freedman population. But

⁴³ Rotter, “Die Stellung des Negers in der islamisch-arabischen Gesellschaft,” 76–77, 131–35. Nevertheless, the legitimist tendency did not disappear, especially in the area of law. Muslim law, and, indeed, the legal systems of all slave-holding societies, have functioned conservatively—that is, toward maintaining as much of the original servile character of the society as possible. Thus, despite the towering legal authority of Quran 24: 30 (“And to those of your slaves who desire a deed of manumission, execute it for them, if ye know good in them, and give them a portion of the wealth of God which He hath given you”), the courts responded more sympathetically to the self-interests of heirs who were in their prime than they did to the piety of patriarchs who were aged or deceased. Later Muslim courts ruled that an owner could not by will alienate more than one-third of his estate. Mez, *The Renaissance of Islam*, 169.

⁴⁴ For a study of highly trained servile entertainers, see Albert Wesselski, “Die gelehrten Sklavinnen des Islams und ihr byzantinischen Vorbilder,” *Archiv Orientalni*, 9 (1937): 353–78. For revolts of free subjects against servile bureaucrats, see A. N. Poliak, “Les révoltes populaires en Egypte à l’époque des Mamelouks et leurs causes économiques,” *Revue des Études Islamiques*, 8 (1934): 251–73. For examples of the conspicuously privileged slave in other cultural settings, see Wallon, *Histoire de l’esclavage dans l’antiquité*, 1: 189; and Mendelsohn, *Slavery in the Ancient Near East*, 71. In slave and former slave societies with strong legitimist traditions, however, a much less dramatic advancement of a low-status person is required to create the scandalous paradox. To produce such a reaction in the United States, for example, it has often been sufficient for a Negro to appear driving a new Cadillac.

⁴⁵ For the use of black slaves in Saharan salt and copper mines, see Ibn Battuta, *Voyages d’ibn Battuta*, trans. and ed. Defremery C. Sanguinetti, 4 (Paris, 1879): 377, 440; and, in gold mines, see Hasan, *Arabs and the Sudan*, 50–63. For a revolt involving thousands of black slaves used in land reclamation projects in southern Iraq, see Alexandre Popovic, *La Révolte des esclaves en Iraq au III^e/IX^e siècles* (Paris, 1976).

⁴⁶ Samuel Sheridan Haas, “The Contributions of Slaves to and Their Influence upon the Culture of Early Islam” (Ph.D. dissertation, Princeton University, 1942), 10–11, 39–52, *passim*; and Michael Brett, ed., *Northern Africa: Islam and Modernization* (London, 1973), 2–3.

blacks fared more poorly than whites as freedmen just as they had as slaves. Even the term *'abīd* still clung to them in freedom, linking them to the lowest stratum of the servile population. Behind this linguistic tie lay the reality that they had moved from the lowest stratum of the servile population to a corresponding position in free society. And *'abīd* no longer denoted their legal status; instead it identified their race. By the ninth century, therefore, the process of enslaving, assimilating, converting, and freeing Negroes had, in a series of Muslim cities from Andalusia to Persia, created a class of blacks who, though legally free, still worked as butchers, bath attendants, and the like, who still toiled in lowly occupations similar to those they had pursued as slaves.⁴⁷ Emancipation did not dramatically change what one saw blacks doing; in popular usage they were still *'abīd*, "slaves."

Despite the general polarization of Muslim society into low-status blacks and high-status whites, no clearly defined color bar emerged. Muslim countries have, therefore, produced some notable examples of the overlapping of racial status groups. Tenth-century Egypt, for instance, had a *de facto* black ruler. In the seventeenth and eighteenth centuries, an elite guard of blacks held the balance of power in Morocco; two centuries later it was ruled by Sultan Mulay Hasan, whose mother was black.⁴⁸ One of the most spectacular examples of black upward mobility is that of Kızlar Agası Beshir (1650–1746), an Abyssinian eunuch purchased for thirty piasters, the price of a first-rate donkey, but who prospered unbelievably during his long tenure as Ottoman secretary of the treasury. By the time of his death, he had amassed a vast fortune and had founded the Mosque of Aga in Istanbul as well as a number of schools and a public library.⁴⁹

Muslim racial attitudes reflect the ambivalence of the system of color stratification in Islamic society, with its inconsistency, at times its seeming lack of color prejudice. Significantly in this respect, a body of Muslim literature emerged that treated blacks sympathetically or defended them against their detractors.⁵⁰ Nevertheless, Muslims lived in a racially stratified society. If they, unlike people in later English-speaking stratified societies, had no clearly defined color bar, if they were correspondingly less disturbed by the occasional appearance of a high-status black, such violations of the prevailing pigmentocracy, or light-skinned dominance, did not happen often enough to discredit the assumptions most people made about skin color and status: light meant superiority, dark meant debasement.⁵¹

Muslim attitudes toward blacks were mixed, but amid their ambivalence one

⁴⁷ Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 114.

⁴⁸ Brown, "Color in North Africa," 191–92.

⁴⁹ Joseph Freiherr von Hammer-Purgstall, *Geschichte des osmanischen Reiches*, 10 vols. (Pest, Austria-Hungary, 1827–35), 8: 70–71.

⁵⁰ For a review of this literature, see Rotter, "Die Stellung des Negers in der islamisch-arabischen Gesellschaft," 10–20. Unfortunately, most of these writers do not defend blacks in general but limit their defense to certain nationalities, especially Abyssinians.

⁵¹ Levy, *Social Structure of Islam*, 61–64; and Gustave E. von Grünebaum, *Medieval Islam: A Study in Cultural Orientation* (2d ed., Chicago, 1954), 199, 109–10. For the development of the idea of "pigmentocracy" as a theory of Latin American race relations, see Alejandro Lipschütz, *El Indioamericanismo y el problema racial en las Américas* (Santiago, 1944).

can detect here and there most of those notions making up that cluster of ideas we recognize as modern Western racial prejudice. As Negroes came to occupy the bottom strata of both free and servile society and as the term *'abīd* came in popular usage to identify a race rather than a legal class, Muslims came to attach to blacks those ideas that Old World peoples had traditionally attached to slaves regardless of their origin. Negroes were thus stereotyped as lazy, lecherous, and prone to lie and steal. And, when humans are treated as domesticated animals, they are sometimes regarded as animallike. Thus, just as the ancients occasionally denied the humanity of slaves, so could Ibn Khaldun, a historian of western Islam, write that "the only people who accept slavery are the Negroes, owing to their low degree of humanity and their proximity to the animal stage."⁵² But not very intelligent animals, a Persian writer thought: "Many have seen that the ape is more capable of being trained than the Negro, and more intelligent." A thirteenth-century Moroccan asserted that the blacks had another quality that seemed to make them especially suited to the debased status that his society generally accorded them. They were "the most stinking of mankind in the armpits and sweat."⁵³ And, according to a popular work on slave buying, blacks were "fickle and careless. Dancing and beating time are engrained in their nature. They say: were the negro to fall from heaven to the earth he would beat time falling."⁵⁴ Thus the most important ideas justifying white dominance had been current in the racially stratified lands of the Mediterranean for several centuries before the northwest Europeans bought their first Negro slaves.

Though Muslims held many disconnected ideas and value judgments that lent stability to a previously evolved pattern of pigmentocracy, the closest they had to a theory of race relations remained the ancient Hamitic myth. During the first century of Islam, the "sons of Ham" had begun their migration to *Bilad al-Sudan*, the land of the blacks; but within their ranks were still a number of light-skinned peoples. Thus, the myth justified slavery more than white superiority, as it had done since early in the first millenium B.C.

The evolution of this tradition is reflected in the genealogies that often follow an account of the curse of Noah. These genealogies purport to show how various nationalities descended from the single family that survived the Flood. They therefore reveal beliefs about which nations have been condemned to perpetual slavery.⁵⁵ At the time of the Muslim conquest of North Africa, most genealogies

⁵² Meyer, *Die Sklaverei im Altertum*, 47–48; and Ibn Khaldun, *An Arab Philosophy of History: Selections from the Prolegomena of Ibn Khaldun of Tunis (1332–1406)*, trans. Charles Issawi (London, 1955), 98.

⁵³ Nasir al-Dīn Tūsī, *Tasavvurāt* (Leiden, 1950), as quoted in A. J. Arberry, *Classical Persian Literature* (London, 1958), 255; and as-Suhaylī, "Er-Rōud el-Unif [*sic*]," as quoted in Edward William Lane, *An Arabic-English Lexicon*, 2 (London, 1872): 1462, s.v. "Sudan."

⁵⁴ Ibn Botlan, "Introduction to the Art of Making Good Purchases of Slaves," as quoted in Mez, *The Renaissance of Islam*, 161.

⁵⁵ Gerhard von Rad has warned against reading traditional Hebrew genealogies literally as expressions of actual kinship between peoples. Rather the language of kinship is used to express the social and political relationships of tenth- to ninth-century Palestine. See von Rad, *Genesis*, 140. In her study of the Tiv people of Nigeria, Laura Bohannon also found that traditional genealogies had little correspondence to actual kinship. Rather, they were constantly being revised to explain changes in then-existing power relationships. See Bohannon, "A Genealogical Charter," *Africa*, 22 (1952): 140, *passim*.

showed the Egyptians, the Berbers, and other conquered peoples as “sons of Ham.” But history did not bear out the truth of the myth in this form. As the conquest swept into southwestern Europe, many of the earlier conquered peoples adopted Islam and distinguished themselves as soldiers and administrators. These nationalities did not look or behave like slaves. Actually they were the products of cultures more urbane and sophisticated than that of their conquerors. By the tenth century, however, the myth had been adjusted to the new reality: Egyptians and Berbers were exempted from the curse. Their reprieve was accomplished by one of two methods. Either the genealogy was revised so that North Africans, though still “sons of Ham,” were descended through some son other than Canaan upon whom the curse had fallen; or the story itself was re-told in such a way that these particular sons of Ham were forgiven for the sin committed by their ancestor.⁵⁶

The blacks fared less well in the real world and thus in the world of myth. In sub-Saharan Africa most of them were protected neither by the peace of Islam nor by powerful indigenous states. Furthermore, most blacks passing through the institution of Muslim slavery did so after the frontiers of Islam had become stabilized and opportunities were more limited than they had been in the years of rapid expansion. Just as the blacks, free and slave, remained more or less permanently clustered at the bottom of the status ladder, so the status-defining myth assumed a relatively fixed and unmistakably racial form. A recounting of the story in the tenth century by the Persian historian, Tabari, is typical of the form it had assumed by the later Middle Ages: “Ham begot all blacks and people with crinkly hair. Yafit [Japheth] all who have broad faces and small eyes (that is, the Turkic peoples) and Sam [also called “Shem” or “Sem,” the mythical ancestor of the “Semites”] all who have beautiful faces and beautiful hair (that is, the Arabs and Persians); Noah put a curse on Ham, according to which the hair of his descendants would not extend over their ears and they would be enslaved wherever they were encountered.”⁵⁷

Popular genealogies often reveal more about existing power relationships than they do about the actual origins and kinship of peoples.⁵⁸ Three centuries before Tabari, for example, the Persians, after their conquest by Muslim armies, had sometimes been sold into slavery and classified as “sons of Ham.”⁵⁹ But then a political upheaval brought the Abbassid caliphs to power and elevated Persians to positions of authority and influence throughout the vast territories of the caliphate.⁶⁰ As a result, by the tenth century Ta-

⁵⁶ Rotter, “Die Stellung des Neger in der islamisch-arabischen Gesellschaft,” 147–49; and *Encyclopaedia of Islam*, s.v. “Ham,” by G. Vajda. Also see Mohammed Tabari, *Chronique . . .*, trans. Louis Dubeux (Paris, 1836), 107–08.

⁵⁷ Abū-Gaʿfar Muhammad b. Garir a-Tabarī, *Taʾrīh ar-rusul waʾl-mulūk*, ed. J. DeGoeje, 1 (Leiden, 1879): 223, as quoted in Rotter, “Die Stellung des Neger in der islamisch-arabischen Gesellschaft,” 147. Though Persian, Tabarī wrote in Arabic and, indeed, is the author of what is regarded as the major Arabic historical work of the Middle Ages.

⁵⁸ Ignaz Goldziher, *Mythology among the Hebrews and Its Historical Development*, trans. Russell Martineau (New York, 1967), 357–59. Also see note 53, above.

⁵⁹ Haas, “Contributions of Slaves to Early Islam,” 62; and Abd al-Husain Zarrinkuls, “The Arab Conquest of Iran and Its Aftermath,” in *Cambridge History of Iran*, 2 (Cambridge, 1968): 29, 42, *passim*.

⁶⁰ *Encyclopaedia of Islam*, new ed., s.v. “Abbasids,” by Bernard Lewis.

barī could perceive the Persians, though they were linguistically unrelated to either the Jews or the Arabs, as “Semites,” a people basking in Noah’s blessing and destined to be served by the now definitely sub-Saharan *Banu Ham*.

UNTIL NEARLY THE END OF THE MIDDLE AGES Christians showed no particular interest in Jewish and Muslim revisions of the Ham story. Their version, to the extent they related the story at all, was still the one that appears in Genesis, sometimes used to justify slavery, but one that remained innocent of racial overtones.⁶¹ In Christian Europe “slave” still meant “Slav,” not “Negro.” But already a development was taking place in the Christian lands of the Mediterranean that would have an enormous impact on the habits of Europeans, on their economy, and on the way they perceived blacks. In the eastern Mediterranean during the First Crusade, Europeans had learned to make sugar, a product that one chronicler described as an “unsuspected and inestimable present from Heaven.”⁶² Sugar production was as labor intensive as it was profitable, creating a strong demand for unskilled, closely supervised, work gangs. Initially, growers were able to answer their needs by investing in the labor supplied by the religious wars, captives sold into slavery, or religious refugees who could be employed under conditions approaching slavery. After the Crusaders were expelled from the Near East, their sugar industry was transferred westward to new areas, especially Cyprus, Crete, Sicily, and parts of Spain. The industry was extended further westward, mostly by Italians, to the Madeira Islands about 1432 and to the Canaries about 1480. It was also Italian merchants who supplied sugar planters with much of their labor, selling them “Slavs” from the Black Sea region.⁶³ Political developments in the Near East greatly affected the sugar industry. On the one hand, the disorder created by a new round of wars in the area and by ruinous Turkish commercial policies, helped Christian planters by eliminating their Muslim rivals.⁶⁴ But, on the other, these same policies threatened trade between

⁶¹ A late thirteenth-century English legal writer, for example, noted that “serfage, according to some, comes from the curse which Noah pronounced against Canaan, the son of his son Ham, and against his issue”; [Andrew Horn] *The Mirror of Justices*, ed. William Joseph Whittaker, Introduction by F. W. Maitland (London, 1895), 77. Also see Piero A. Milani, *La schiavitù nel pensiero politico dai Greci al basso Medio Evo* (Milan, 1972), 244, 292, 300, 309, 316, 355–57, 376–77, 377 n. 16.

⁶² Charles Verlinden, *Les origines de la civilisation atlantique de la Renaissance à l’Âge des Lumières* (Paris, 1966), 167.

⁶³ Origo, “The Domestic Enemy: The Eastern Slaves in Tuscany,” 361 n. 44; Charles Verlinden, “Aspects de l’esclavage dans les colonies médiévales italiennes,” *Hommage à Lucien Febvre*, 2 (Paris, 1953): 103; and Noel Deerr, *The History of Sugar*, 1 (London, 1949): chap. 8 *passim*, esp. 74, 76–80, 100, 115. Some sugar had been produced in Spain, mainly in the Costa del Sol region, since the Muslims had introduced it in the tenth century. It had flourished in conjunction with a secondary industry, the manufacture of various kinds of jams and concentrates for soft drinks made from local fruits. Although with the Reconquest and the re-establishment of Christianity many people favored wine over Muslim-type soft drinks, certain of these preparations such as grenadine and sarsaparilla continued to be popular. Knowledge of the techniques for making jams and soft-drink concentrates continued to be regarded as a desirable accomplishment for slave women. Edmund O. von Lippmann, *Geschichte des Zuckers seit den ältesten Zeiten bis zum Beginn der Rubenzucker-Fabrikation* (Berlin, 1929), 241; and Ruth Pike, *Aristocrats and Traders: Sevillian Society in the Sixteenth Century* (Ithaca, N.Y., 1972), 177–78.

⁶⁴ W. Heyd, *Histoire du commerce du Levant au moyen âge*, 2 (Leipzig, 1923): 276 ff.; Poliak, “Révoltes populaires en Egypte,” 253–254.

the Black Sea and the Mediterranean and, hence, the labor supply of Christian planters. The price of sugar climbed, but so did the price of slaves.⁶⁵

In 1444 Diniz Dias, his caravel racing before the constant north wind, bypassed the fearful Saharan inferno and dropped anchor before the green land of Guinea. There the Portuguese found peoples of many languages and customs cultivating fields of rice, millet, and yams. Guinea, or more strictly Upper Guinea, is a relatively narrow strip of tropical rain forest extending along the southern and southwestern coast of the large western bulge of Africa. Even before the coming of the Europeans these peoples had suffered slave raids. For the great Sudanese savanna to the north, separating the rain forest from the desert and forming an east-west axis across the continent, may have played a role in the history of Guinea similar to that played by the Eurasian steppe in the history of Caucasia.

Higher and less humid than Guinea, Sudan sustained a healthier population that enjoyed a greater prosperity due to better conditions for agriculture and stock raising. Like the Eurasian steppe, medieval Sudan had been the scene of great empire building. These empires, however, had never extended into the relatively inaccessible areas of the rain forest with its comparatively poor and isolated village populations. Large-scale cavalry warfare, which built and overthrew the empires of the savanna, was ineffective in the forest lowlands. Furthermore, the lowlands were infested with the sleeping sickness-bearing tsetse fly, which is dangerous to man but deadly to horses.⁶⁶ Nevertheless, small-scale raids into the rain forests, especially slave raids, were feasible, since the raiders could usually be confident that they would face not armies but peasant communities that, because of the local nature of their political institutions, could not call on outside help.⁶⁷

Moreover, the conversion of Sudanese governing elites to Islam had results similar to those produced by the emergence of Christian governments in medieval Europe. On the one hand, the support of a well-organized, literate religion strengthened the position of kings, enabling them to offer greater protection to their own subjects. But, on the other, religion provided a rationale for slave

⁶⁵ Ashtor, *Histoire des prix et des salaires dans l'orient médiéval*, 460–61, 498–504; and Karl Schneider, "Der Sklavenhandel im mittelalterlichen Italien," *Zeitschrift für Sozialwissenschaft*, 20 (1907): 238.

⁶⁶ Jack Goody, "Introduction," in Jack Goody and Kwame Arhin, eds., *Ashanti and the Northwest* (Legon, Ghana, 1955), 82–83.

⁶⁷ Bolanle Awe, "Empires of the Western Sudan: Ghana, Mali, Songhai," in J. F. A. Ajayi and Ian Espie, eds., *A Thousand Years of West African History* (Ibadan, Nigeria, 1965), 57–59; and Christopher Fyfe, "Peoples of the Windward Coast, A.D. 1000–1800," in *ibid.*, 149–50, 158–59. Several scholars have held that the inevitable human disasters connected with building the medieval Sudanese empires caused a southward drift of population into the less favorable environment of the rain forests and thus contributed substantially to the cultural diversity of Guinea. See Akin Mabogunje, "The Land and Peoples of West Africa," in J. F. A. Ajayi and Michael Crowder, eds., *History of West Africa*, 1 (New York, 1972): 28; Jacques Richard-Molard, *Afrique occidentale française* (Paris, 1949), 108. Walter Rodney, who based his own work on that of Richard-Molard and Antonio Mendes Correia, the anthropologist, reached similar conclusions. See Rodney, *A History of the Upper Guinea Coast, 1545–1800* (Oxford, 1970), 5–6. Raymond Mauny, however, has had the "impression" that most of the peoples of Guinea had lived a very long time in those locations where they are first noted in Arabic and Portuguese sources. See Mauny, *Tableau géographique de l'ouest africain au moyen âge d'après les sources écrites, la tradition et l'archéologie* (Dakar, 1961), 448.



Figure 1: The Mossi prince, Boukary, later ruler of the kingdom, and his horsemen on a slave-raiding expedition. Photograph from a wood engraving, taken from Captain L. G. Binger's description of his two years in Africa; Binger, *Du Niger au Golfe de Guinée par le pays de Kong et le Mossi*, 1 (Paris, 1892): 455.

raids. With the growing volume of the trans-Saharan slave trade and, especially, with the opening of the Atlantic trade, Sudanese rulers showed an increasing tendency to proclaim a *jihad*, or holy war, against *Kaffirs* (unbelievers) as well as against Muslim heretics. Like similar movements in Europe, such as the *Drang nach Osten*, the *Reconquista*, or the Crusades, these campaigns delivered thousands of victims to the slave traders.⁶⁸

With the opening of South Atlantic navigation, the relative isolation of the peoples of Guinea ended. They found themselves between the hammer of Portuguese maritime power and the anvil of the Muslim states. Furthermore, Guinea, like the Caucasus region and the slave export areas of the Balkans, was a kaleidoscope of local cultures in which slave traders had no trouble finding ethnic antagonism from which they could profit.⁶⁹ Just as the Italian traders

⁶⁸ J. Spencer Trimingham, *Islam in West Africa* (Oxford, 1959), 28–30; Mauny, *Tableau géographique de l'ouest africain au moyen âge*, 541–42; and Rodney, *Upper Guinea Coast, 1545–1800*, 236. The Sudanese poet, Mohammed Bello, writing Hausa verse in Arabic script, voiced sentiments that would not have been unusual among the Crusaders, Junkers, or conquistadores of medieval Europe: "Brethren we thank God; / We perform acts of faith and prayer / Even holy war for Thee the Exalted One: / We slew the breed of dogs, . . . / We have slain the heathen." See J. H. Greenberg, "Hausa Verse Prosody," in *Journal of the American Oriental Society*, 69 (1949): 127.

⁶⁹ An official for the Royal Africa Company in 1726, noting the variety of languages along the Gambia such that the people on one side of the river could not understand those on the other, concluded that this situation was "no small Happiness to the Europeans who go thither to trade for slaves." Since Gambians, who were "naturally very lazy, abhorre slavery," the business could be quite dangerous. But, by "having some of every sort on board," there was "no more likelihood of their succeeding in a plot than finishing the Tower of Babel." William Smith, *A New Voyage to Guinea* (London, 1744), 28. Another observer noted that there were seven or eight languages spoken along a sixty-mile stretch of the Gold Coast and that three or four of these were not mutually comprehensible. William Bosman, *A New and Accurate Description of the Coast of Guinea . . .* (1704; 4th English ed., London, 1967), 130.

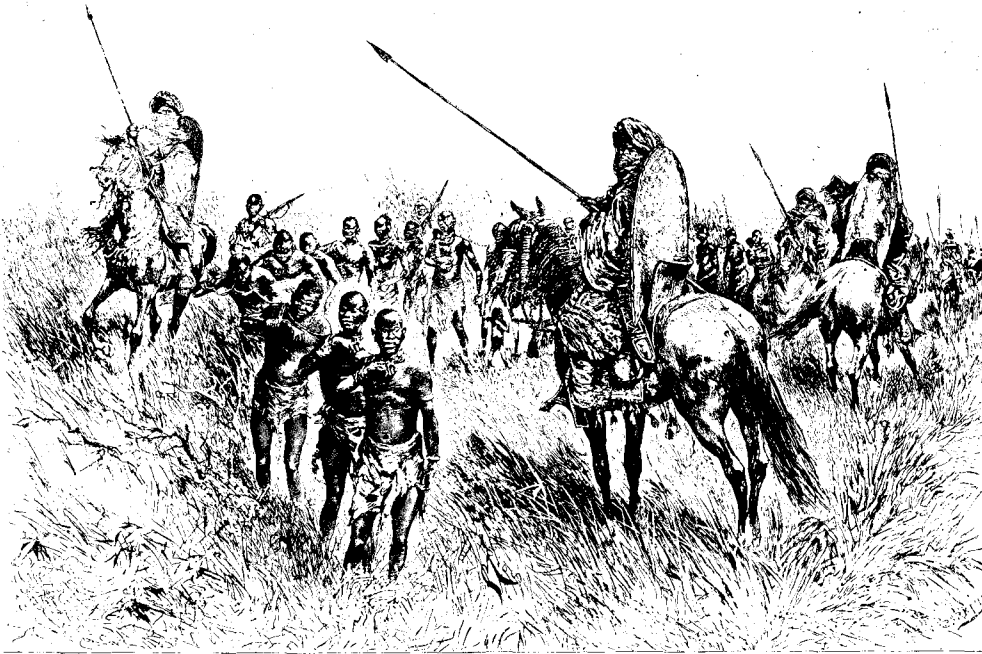


Figure 2: The Mossi horsemen returning with their captive slaves taken during a raid on noncentralized African peoples. During his exploration of Africa, Binger met Boukary in 1888 and described two such raiding parties conducted by the prince. Photograph from a wood engraving, taken from Binger, *Du Niger au Golfe de Guinée*, 471.

were furnishing arms to the Crimean Tartars in exchange for the captives taken in the raids against the Slavic and Caucasian peoples, so the Portuguese established a similar relationship with the Bijagos (or Bissagos) of Guinea.⁷⁰ The Europeans had thus discovered a vast source of black “Slavs”—a source that lay, as it turned out, not far from the major trade-wind, sailing route between Europe and the Americas.

At the same time, the Black Sea trade, the principal source of white slaves, became increasingly constricted. Nine years after the Portuguese opened up the maritime route to Guinea, Constantinople fell to the Turks, eliminating the last relic of Christian control over the Straits. To be sure, the Black Sea trade continued for a time but only on terms laid down by the Turks.⁷¹ Turkish restrictions on the buying and selling of Muslims not only sharply reduced one important source of light-skinned slaves but also, insofar as some Italian merchants were concerned, stood the religious justification for slavery on its head. Cautious about dealing in Muslims, Genoese and Venetian traders furnished the victorious Ottomans with ready cash for their surplus Christians, including Christian monks. These payments were not made for the pious medieval purpose of ransoming brothers in Christ. Inspired by a more modern spirit of enterprise, Ital-

⁷⁰ Rodney, *Upper Guinea Coast, 1545–1800*, 104. As early as 1460–62, according to a contemporary report, the Portuguese procured slaves through Muslim middlemen. See G. R. Crone, ed., *The Voyages of Cadamosto and Other Documents*, Hakluyt Society Publications, 2d ser., no. 80 (London, 1937), 18. Also see G. I. Bratianu, *Recherches sur le commerce génois dans la Mer Noire* (Paris, 1929), 289; and Verlinden, “Aspects de l’esclavage dans les colonies médiévales italiennes,” 94–95, *passim*.

⁷¹ A. H. Lybyer, “The Ottoman Turks and the Routes of Oriental Trade,” *English Historical Review*, 30 (1915): 580–84.

ian merchants snapped up these victims of Turkish expansion for use on the sugar plantations of their island colonies in the eastern Mediterranean or shipped them to the western Mediterranean where they were sold in competition with blacks whom the Portuguese had begun to bring up from Guinea.⁷² In the long run, however, the commerce of the Italian cities did not prosper in the lands controlled by the Turks. In the second quarter of the fifteenth century 91 percent of the slaves in Genoa were of Black Sea origin; by the last quarter of the century this figure had fallen to 26 percent.⁷³

Not only was the flow of slaves from the Black Sea area to the western Mediterranean becoming increasingly constricted, but also the largest single source of white slaves in the mid-fifteenth century, the Russians, virtually dried up. In 1462 Ivan the Great became grand duke of Moscow. His accession marked the end of a long period of political fragmentation as well as the emergence of a viable state, which, like the Ottoman state, could offer its subjects some protection against slave raids.⁷⁴ Of the Black Sea slaves being sold at Genoa, the proportion that was Russian fell from 41 percent in the second quarter of the fifteenth century to 18 percent in the last quarter. Furthermore, the rise in the median age of Russian slaves being sold in the last quarter suggests that many were resales who had been in Italy for some time and that among the freshly taken captives the proportion of Russians was even lower.⁷⁵

With this turn of events the reputation of Slavs began to improve. At the beginning of the fifteenth century the Tunisian historian, Ibn Khaldun, could still write that Negroes had character traits "close to those of dumb animals. It has even been reported that most of the Negroes of the first zone [the tropics] dwell in caves and thickets, eat herbs, live in savage isolation and do not congregate, and eat each other. The same applies to the Slavs."⁷⁶ But, before the century was over, as a victorious Slavic nation swept out onto the steppes and began the conquest of the Eurasian heartland, the image of Slavs began to change. Sugar production was expanding rapidly in the Atlantic islands, and the cane fields were being filled with "Slavs" who came from Guinea instead of the Crimea. The term "slave" was losing all of its psychological connection to Eastern Europe, just as centuries before the term "Canaanite" had lost all connection with Canaan.

As historical events redirected the slave trade, as European slavery entered what the leading authority on medieval slavery has called its "Negro" period,⁷⁷

⁷² I. Sukasov, "Documents récemment découverts datant de la fin du XIV^e siècle et concernant les Bulgares de la Macédoine vendus comme esclaves," *Makedonski pregled*, 7 (1932): 236; Charles Verlinden, "La Crète, débouché et plaque tournante de la traite des esclaves aux XIV^e et XV^e siècles, in *Studi in onore di Amintore Fanfani*, 3 (Milan, 1962): 630, *passim*; and communication from Charles Verlinden, June 15, 1979.

⁷³ For these estimates, see Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV*, 61.

⁷⁴ George Vernadsky, *Russia at the Dawn of the Modern Age*, vol. 4 of George Vernadsky and Michael Karpovich, eds., *A History of Russia* (New Haven, 1959), 1-12, 96-101.

⁷⁵ Gioffrè, *Il mercato degli schiavi a Genova nel secolo XV*, 19.

⁷⁶ Ibn Khaldun, *The Muqaddimah: An Introduction to History*, trans. Franz Rosenthal, 1 (New York, 1958): 168-69.

⁷⁷ Charles Verlinden, "Schiavitù ed economia nel Mezzogiorno agli inizi dell'età moderna," *Annali del Mezzogiorno*, 3 (1963): 37. Also see Charles Verlinden, "Esclavage noir en France méridionale et courants de traite en Afrique," *Annales du Midi*, 78 (1966): 335-443; and Vicenta Cortes, *La esclavitud en Valencia durante el reinado de los Reyes Católicos, 1479-1516* (Valencia, 1964), 16.

Christians began to look at blacks in ways that had been characteristic of racially stratified Muslim countries for some seven centuries. Perhaps the bare beginnings of this change appear in a report that a mid-fifteenth-century Portuguese chronicler made to Prince Henry the Navigator. He wrote that a “noble Moor” captured on the Saharan coast had proposed through the Arabic interpreter that, if he were allowed to return home, his ransom would be paid in “black Moors.” “Here you must note,” the chronicler told the prince, “that these blacks were Moors like the others, though their slaves, in accordance with ancient custom, which I believe to have been because of the curse which, after the Deluge, Noah laid upon his son Cain [*sic*], cursing him in this way—that his race should be subject to all the other races of the world.” But, when it turned out that the ten blacks that were actually delivered in payment of his ransom were not in fact Muslims, the writer piously added: “For though they were black, yet had they souls like the others, and all the more as these blacks were not of the lineage of the Moors but were Gentiles, and so the better to bring into the path of salvation.”⁷⁸ Blacks were still “Gentiles.” In time, Europeans would call them by other names, names less connected with religion.

IN THE TWO CENTURIES AFTER COLUMBUS, race relations in some of the recently discovered lands took a new turn. To the predominantly Negro, but racially mixed, slavery of the Christian and Muslim Old World was added the exclusively Negro slavery of the Americas. In their quest for an explanation for this development, a number of scholars have stressed the importance of the meaning that Old World peoples attributed to color. To these peoples angels were white, the devil was black. They associated white or lightness with such positive values as virtue, chastity, and purity and linked black or darkness with ignorance, filth, death, and sorrow. It has been suggested that such color values may have influenced early settlers of the American colonies so that, when confronted with a racially mixed labor force, colonists tended to favor whites over blacks, establishing a pattern of discrimination that resulted in the emergence of a system of slavery that was exclusively Negro.⁷⁹

But this hypothesis fails to account for the stable relationship that had long existed in the Old World between racially mixed slavery on the one hand and color prejudice on the other. Furthermore, after the brief “Negro period” of slavery in Europe in the second half of the fifteenth century, white slavery revived there—in the face of essentially the same color prejudice.⁸⁰ While any

⁷⁸ Gomes Eannes de Azurara, *The Chronicle of the Discovery and Conquest of Guinea*, Hakluyt Society Publications, 1st ser., no. 95 (London, 1895), 54–55.

⁷⁹ Philip Mason, *Prospero's Magic: Some Thoughts on Class and Race* (1962; 2d ed., Westport, Conn., 1975), 58, 77, *passim*; Kenneth J. Gergen, “The Significance of Skin Color in Human Relations,” in Franklin, *Color and Race*, 114, 120; Talcott Parsons, “The Problem of Polarization on the Axis of Color,” in *ibid.*, 358; Jordan, *White over Black*, ix–x, 9, 41, *passim*; Carl N. Degler, *Neither Black nor White: Slavery and Race Relations in Brazil and the United States* (New York, 1971), 208–12; and Joel Kovel, *White Racism: A Psychohistory* (New York, 1970), 232.

⁸⁰ Lybyer, *Ottoman Empire in the Time of Suleiman the Magnificent*, 45–61, *passim*; Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Phillip II*, trans. Siân Reynolds, 2 (New York, 1973): 754–55, 841–42; Antonio Domínguez Ortiz, “La esclavitud en Castilla durante la edad moderna,” *Estudios de historia social de España*, 2 (1962): 380–81, 381 n. 24; von Hammer-Purgstall, *Geschichte des osmanischen Reiches*, 3: 167, 5: 321, 6: 154; and Paul W. Bamford, “Slaves for the Galleys of France, 1665–1700,” in James Ford Bell, ed., *Mer-*

simple explanation for a complex development is risky, a more promising explanation of the rise of exclusively black slavery in the New World appears to be the differences in mortality rates of the various racial populations of the early modern world. According to Philip D. Curtin, "statistics now available suggest" that the death rate of blacks in the New World was "about one-third as high as that for European newcomers."⁸¹ Investment in a black slave was, therefore, far safer and more profitable in the Americas than investment in a white one. Thus, in the New World, during the sixteenth and seventeenth centuries, black slavery replaced the various forms of white servitude. In the Mediterranean world, however, where conditions were healthier for whites, slave dealers could continue to buy and sell blacks and whites with egalitarian evenhandedness, seemingly oblivious to the fact that their world of religious ideas had been populated by white angels and a black devil for thousands of years.

During these same two centuries when slavery in the New World was becoming exclusively black, several patterns of race relations emerged. On the one hand, there was a discernible legitimist impulse. The Iberian nations, beginning in the Portuguese colonies in India (and perhaps influenced by the caste system of that country), established a system of racial "castas," in which pure-blooded whites were the most privileged group. But this system failed throughout the Portuguese and Spanish empires, where the European settlers were overwhelmingly males. Even when white women finally began arriving in the colonies in greater numbers, they found themselves confronted by a strongly established paternalistic culture, one of the characteristics of which is a marked sexual division of labor: women strive to become specialists in virtue, men in sin. The Portuguese crown and benevolent societies, for example, strove mightily to establish the legitimate white family, even to the extent of subsidizing the dowries of chaste white girls. But, while male colonists showed considerable appreciation for the ideal that these young ladies reflected, as evidenced by the popularity of the Marian cult, they also showed a definite reluctance to give up their black and brown concubines. As a result, many splendid young white women, unable to find husbands, entered convents or returned to Portugal, taking their virtue with them.⁸²

chants and Scholars: Essays in the History of Exploration and Trade (Minneapolis, 1965), 171–91. Furthermore, the racially mixed slavery of the Mediterranean continued for at least a generation after the disappearance of black slavery from the Americas. According to Reuben Levy, Circassians and blacks were still being sold on the slave block in Istanbul in 1908; Levy, *The Social Structure of Islam* (Cambridge, 1957), 88. And, in 1948, the United Nations Declaration of Human Rights noted the persistence of this institution in the Near East; see Müller, "Sklaven," 77.

⁸¹ Curtin, "The Atlantic Slave Trade, 1600–1800," in Ajayi and Crowder, *History of West Africa*, 253. How little color values contributed to the shaping of American labor systems is further suggested by the simultaneous decline in Indian slavery in the same lands where there was a rise of exclusively black slavery and a decline of white servitude. See Almon Wheeler Lauber, *Indian Slavery in Colonial Times within the Present Limits of the United States* (New York, 1913). Also see Bamford, "Slaves for the Galleys of France," 185, 190.

⁸² A. J. R. Russell-Wood, *Hidalgos and Philanthropists: The Santa Casa da Misericórdia of Bahia, 1550–1755* (Berkeley and Los Angeles, 1968), 32–33, 179–83. Brazil's paternalistic culture, moreover, may have inhibited the bourgeois process of capital accumulation. Because aging patriarchs could count more sins than legitimate heirs, they sometimes willed property—property that in other societies would have perpetuated or increased the economic power of a family—to trust funds for financing masses for the dead. João de Mattos de Aguiar, who died in 1700, furnishes an extreme example of this tendency. His will provided for eleven thousand

The legitimate white family was numerically weak in the Iberian colonies, and its advocates suffered setbacks, as bastards—the brown-skinned sons of concubines and slaves—filled the ranks of the militia and sometimes served as officers and bureaucrats as well. What has replaced “castas” in the Iberian empires is a *café con leche* society, consisting of a racial continuum in which whites are clustered toward the top both socially and economically while blacks are clustered toward the bottom, but a society without clearly defined racial frontiers.⁸³ This pattern has long existed in many Muslim countries and, prior to the great discoveries, apparently had already been established in those parts of Portugal and Castile that had long remained under Islamic influence.⁸⁴ The Muslim-Iberian system involves a subtle interplay between the resources of each individual and the white racial prejudices of society that rewards the light-skinned and penalizes the dark. Its flexibility enables an occasional Negro to rise high and allows more than an occasional white to remain poor.

In the seventeenth century, as the system of “castas” began to disintegrate in the Iberian colonies, it was picked up by the northern European powers and applied more successfully in their colonies, where the sex ratio among white settlers was more balanced.⁸⁵ a white, colored, black, three-status system in the West Indies, and a biracial, two-status system in North America. The status systems established by the north European empires incorporated several features similar to the caste system of India, including status group endogamy or in-group marriage, the association of each status group with a particular kind of economic activity, and a corresponding degree of access to political power.⁸⁶ Yet the racial status hierarchies of these societies have never become as firmly established as has the caste system of India. Despite the most valiant efforts of that great school of status-sustaining mythology and taboo, the legitimate white family, the quasi-caste systems of the New World and South Africa have never been upheld by such an elaborate ideology as that which lends a certain stability to the caste system of India. Thus, low-status people of these societies do not to the same degree internalize caste values. Nor have the quasi-caste systems ever become self-policing. The perpetuation of patterns of caste behavior in these coun-

masses annually, at a cost of two hundred réis each, to be said for his soul and those of his parents and grandparents. *Ibid.*, 167–68. For the failure of legitimacy in other parts of the empire, see Charles Ralph Boxer, *Race Relations in the Portuguese Empire* (Oxford, 1963), 49–53, 116–17.

⁸³ Boxer, *Race Relations in the Portuguese Empire*, 50, 57, 73–76, 83–84. For the breakdown of the system of “castas” but the persistence of color prejudice, see Florestan Fernandes, *The Negro in Brazilian Society*, trans. Jacqueline D. Skiles *et al.* (New York, 1969), 12, 135, 168, 451 n. 20; Gonzalo Aguirre Beltrán, *La población negra de México* (Mexico City, 1972), 85, 93, 154, 198, 224–33, 267–80; and Franklin W. Knight, “Cuba,” in David W. Cohen and Jack P. Greene, eds., *Neither Slave nor Free: The Freedmen of African Descent in the Slave Societies of the New World* (Baltimore, 1972), 280. For an interpretation that stresses the differences between the American racial status groups and the East Indian caste system, see Magnus Mörner, *Race Mixture in the History of Latin America* (Boston, 1967), 53–56.

⁸⁴ E. Levi-Provençal, *Histoire de l’Espagne musulmane*, 3 (Paris, 1953): 186, 208; Domínguez Ortiz, “La esclavitud en Castilla durante la edad moderna,” 372.

⁸⁵ Herbert Moller, “Sex Composition and Correlated Cultural Patterns of Colonial America,” *William and Mary Quarterly*, 2 (1945): 131–37.

⁸⁶ H. Hoetink, *Caribbean Race Relations: A Study of Two Variants*, trans. Eva M. Hooykaas (New York, 1967); and Degler, *Neither Black nor White*. These studies provide an excellent comparison of the two basic patterns of New World race relations, though I disagree with some of the conclusions reached by each author.

tries, unlike in India, has continued to depend upon the heavy-handed intervention of a strong state, ultimately upon the pistols and sabers of militia companies manned by the white-skinned sons of the "uptight" legitimate family.

Despite the New World's history of exclusive black slavery, patterns of race relations have arisen there that share an underlying common feature with those of the Muslim world of the Near East and North Africa: each of these societies is a pigmentocracy, ruled by people with light skin. This racial distribution of power, established centuries ago by the clash of arms, is maintained by a system of color values that permits a governing elite to define eligibility to power and privilege in its own image. Such color values are expressed in a cluster of distance-creating ideas, ideas that attach characteristics to the Negro that were once attached to slaves of any origin and that make a caricature of the physical and cultural traits of sub-Saharan Africans. Such caricatures inevitably imply that the debasement of blacks is the result of their race and culture rather than their history of captivity and exploitation. These perceptions, which emerged in the racially stratified countries of the Muslim Mediterranean during the centuries immediately prior to the great discoveries, created neither racial stratification in the Old World nor exclusively black slavery in the New. Historically, they arose as an attempt to explain and justify already existing social relations, social relations that were the result of the physical processes of history. Their function is thus conservative rather than creative. That is, they function to conserve old social relationships rather than to create new ones.

But, while the distance-creating perceptions of blacks were by no means new, most of them going back at least to the eleventh century, these old ideas were now operating within a new social environment, within the ethos of emerging capitalism. This new environment made an important difference. Some of the world's greatest fortunes were being won from the extraction of precious metals and from the production of plantation commodities, particularly sugar. Although these industries were not confined to the New World, the Americas presented entrepreneurs not only with their greatest opportunities but also with their most acute problem: how to persuade people to work for bare subsistence in mines and on plantations located in a country of "open resources,"⁸⁷ where there was an abundance of land often free for the taking? How could entrepreneurs control other people's impulse to "get ahead" in a land of boundless opportunity? Slavery helped. But so have the various "color bars" and patterns of color prejudice that restrict upward mobility and, hence, function to maintain an adequate supply of cheap labor, making possible higher profits.

To be sure, in those parts of the Americas where the white man continued to deal with the black man as his, or somebody's, household servant, the more typical pattern of Old World racial prejudice still persisted—with all of its inconsistencies and its admixture of paternalism. But, wherever color prejudice has been

⁸⁷ For the theory that, "leaving militarism and other secondary causes out of consideration," slavery flourishes only in situations of "open resources" or frontier conditions, see H. J. Nieboer, *Slavery as an Industrial System* (The Hague, 1900), 306, 347, *passim*. The "Nieboer thesis" is further developed by W. Kloosterboer; see her *Involuntary Labour since the Abolition of Slavery* (Leiden, 1960).

reinforced by the profit motive, wherever relations with the blacks have been conditioned not only by face to face contact but also by the pressure of distant and impersonal markets, white prejudices have taken on such an immediacy, a consistency, a neurotic intensity that a number of scholars have understandably mistaken this attitude for an "irrational" psychological phenomenon rather than a discrete historical one.⁸⁸ In certain advanced capitalist countries, furthermore, these ideas about blacks have been transmitted to each successive generation by means of a legitimate white family, which, by uniting an appeal to immediate self-interest with moral zeal, has elevated an ancient cluster of perceptions and myths to the level of an impassioned ideology.

This investigation, thus, does not bear out the belief that patterns of race relations were shaped by pre-existing prejudices of whites. Rather, it supports the proposition that patterns of both race relations and prejudice are determined by power relationships. Inevitably, power relationships change, often for reasons that have little to do with what people think about status; and, as they change, old patterns of thought give way to new ones. Myths become transformed. Sometimes they are even discarded.

⁸⁸ Especially see H. Hoetink, *Slavery and Race Relations in the Americas: Comparative Notes on Their Nature and Nexus* (New York, 1973), 51, *passim*. Also see Kovel, *White Racism: A Psychohistory*, 95, *passim*; Degler, *Neither Black nor White*, 211, *passim*; Jordan, *White over Black*, vii, x, 9, *passim*.

Time, Space, and the Evolution of Afro-American Society on British Mainland North America

IRA BERLIN

TIME AND SPACE ARE THE USUAL BOUNDARIES of historical inquiry. The last generation of slavery studies in the United States has largely ignored these critical dimensions but has, instead, been preoccupied with defining the nature of American slavery, especially as compared with racial bondage elsewhere in the Americas. These studies have been extraordinarily valuable not only in revealing much about slave society but also in telling a good deal about free society. They have been essential to the development of a new understanding of American life centered on social transformation: the emergence of bourgeois society in the North with an upward-striving middle class and an increasingly self-conscious working class and the development of a plantocracy in the South with a segmented social order and ideals of interdependence, stability, and hierarchy. But viewing Southern slavery from the point of maturity, dissecting it into component parts, comparing it to other slave societies, and juxtaposing it to free society have produced an essentially static vision of slave culture. This has been especially evident in the studies of Afro-American life. From Stanley M. Elkins's *Sambo* to John W. Blassingame's Nat-Sambo-Jack typology, scholars of all persuasions have held time constant and ignored the influence of place. Even the most comprehensive recent interpretation of slave life, Eugene D. Genovese's *Roll, Jordan, Roll*, has been more concerned with explicating the dynamic of the patriarchal ideal in the making of Afro-American culture than in explaining its development in time and space. None of the histories written since World War II has equaled the temporal and spatial specificity of U. B. Phillips's *American Negro Slavery*.¹

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¹ Stanley Elkins, *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago, 1959); John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1972); Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); and Ulrich B. Phillips, *American Negro Slavery* (New York, 1918). For a historical perspective on post-World War II scholarship on slavery, see David Brion

Recent interest in the beginnings of slavery on the mainland of British North America, however, has revealed a striking diversity in Afro-American life. During the seventeenth and eighteenth centuries, three distinct slave systems evolved: a Northern nonplantation system and two Southern plantation systems, one around Chesapeake Bay and the other in the Carolina and Georgia lowcountry. Slavery took shape differently in each with important consequences for the growth of black culture and society. The development of these slave societies depended upon the nature of the slave trade and the demographic configurations of blacks and whites as well as upon the diverse character of colonial economy. Thus, while cultural differences between newly arrived Africans and second and third generation Afro-Americans or creoles² everywhere provided the basis for social stratification within black society, African-creole differences emerged at different times with different force and even different meaning in the North, the Chesapeake region, and the lowcountry.³ A careful examination of the diverse development of Afro-American culture in the colonial era yields important clues for an understanding of the full complexity of black society in the centuries that followed.

THE NATURE OF SLAVERY AND THE DEMOGRAPHIC BALANCE of whites and blacks during the seventeenth and first decades of the eighteenth centuries tended to incorporate Northern blacks into the emerging Euro-American culture, even as

Davis, "Slavery and the Post-World War II Historians," *Daedalus*, 103 (1974): 1-16; and, on the importance of temporal change, see Herbert G. Gutman, "Slave Culture and Slave Family and Kin Network: The Importance of Time," *South Atlantic Urban Studies*, 2 (1978): 73-88.

² I have used these terms synonymously. Both are mined with difficulties. "Afro-American" has recently come into common usage as a synonym for "black" and "Negro" in referring to people of African descent in the United States. Although "creole" generally refers to native-born peoples, it has also been applied to people of partly European, but mixed racial and national, origins in various European colonies. In the United States, "creole" has also been specifically applied to people of mixed but usually non-African origins in Louisiana. Staying within the bounds of the broadest definition of "creole" and the literal definition of "Afro-American," I have used both terms to refer to black people of native American birth.

³ As used in this essay, the concept of acculturation or creolization does not mean the liquidation of a culture, only its transformation. African culture transported to the New World was not lost or destroyed but transformed. The transformation of Africans to Afro-Americans entailed the joining together of a variety of distinctive African cultures as well as the compounding of those cultures with various European and native American ones to create a new cultural type: the Afro-American. Scholars have only begun to study the making of Afro-American culture; therefore, any judgment about its nature and the process of its creation must be tentative and incomplete. I would emphasize that "Africans" and "creoles" as used here do not represent autonomous categories, if for no other reason than African and creole people were connected by ties of blood and kinship. Instead, these categories are used as two poles within a range of an historical experience that was varied and overlapping. The process of creolization was not always synchronized with generational change. Beginning with Melville J. Herskovits's *The Myth of the Negro Past* (New York, 1941), scholars have produced a wide-ranging theoretical literature on the question of cultural transformation of African people in the New World. For some that have been most useful for this essay, see Sidney W. Mintz and Richard Price, *An Anthropological Approach to the Caribbean Past* (Philadelphia, 1976); Melville J. Herskovits, "Problem, Method, and Theory in Afro-American Studies," *Phylon*, 7 (1946): 337-54; M. G. Smith, *The Plural Society in the British West Indies* (Berkeley and Los Angeles, 1965), and "The African Heritage in the Caribbean," in Vera Rubin, ed., *Caribbean Studies* (Seattle, 1960), 34-45; H. Orlando Patterson, "Slavery, Acculturation, and Social Change: The Jamaican Case," *British Journal of Sociology*, 17 (1966): 151-64; and Edward Brathwaite, *The Development of Creole Society in Jamaica, 1770-1820* (London, 1971), and "Caliban, Ariel, and Unprospero in the Conflict of Creolization: A Study of the Slave Revolt in Jamaica in 1831-32," in Vera Rubin and Arthur Tuden, eds., *Comparative Perspectives on Slavery in New World Plantation Societies*, Annals of the New York Academy of Sciences, no. 292 (New York, 1977), 41-62.

whites denied them a place in Northern society.⁴ But changes in the character of the slave trade during the middle third of the eighteenth century gave new impetus to African culture and institutions in the Northern colonies. By the American Revolution, Afro-American culture had been integrated into the larger Euro-American one, but black people remained acutely conscious of their African inheritance and freely drew on it in shaping their lives.

Throughout the colonial years, blacks composed a small fraction of the population of New England and the Middle Colonies. Only in New York and Rhode Island did they reach 15 percent of the population. In most Northern colonies the proportion was considerably smaller. At its height, the black population totaled 8 percent of the population of New Jersey and less than 4 percent in Massachusetts and Connecticut. But these colony-wide enumerations dilute the presence of blacks and underestimate the importance of slave labor. In some of the most productive agricultural regions and in the cities, blacks composed a larger share of the population, sometimes constituting as much as one-third of the whole and perhaps one-half of the work force.⁵ Although many Northern whites never saw a black slave, others had daily, intimate contact with them. And, although some blacks found it difficult to join together with their former countrymen, others lived in close contact.

The vast majority of Northern blacks lived and worked in the countryside. A few labored in highly capitalized rural industries—tanneries, salt works, and iron furnaces—where they often composed the bulk of the work force, skilled and unskilled. Iron masters, the largest employers of industrial slaves, also were often the largest slaveholders in the North. Pennsylvania iron masters manifested their dependence on slave labor when, in 1727, they petitioned for a reduction in the tariff on slaves so they might keep their furnaces in operation. Bloomeries and forges in other colonies similarly relied on slave labor.⁶ But in an overwhelmingly agrarian society only a small proportion of the slave population engaged in industrial labor.

Like most rural whites, most rural blacks toiled as agricultural workers. In southern New England, on Long Island, and in northern New Jersey, which contained the North's densest black populations, slaves tended stock and raised crops for export to the sugar islands. Farmers engaged in provisioning the West Indies with draft animals and foodstuffs were familiar with slavery and had easy

⁴ In the discussion of the Chesapeake region and the lowcountry, scholars have employed the term "Anglo-American" to refer to the culture of white people. Because of the greater diversity of origins of white peoples in the Middle Colonies, the term "Euro-American" seems more applicable to white culture in the North.

⁵ For a collection of the relevant censuses, see William S. Rossiter, *A Century of Population Growth* (Washington, 1909), 149–84. Also see Robert V. Wells, *The Population of the British Colonies in America before 1776: A Survey of Census Data* (Princeton, 1975), 69–143, and Wells's correction of the 1731 enumeration, "The New York Census of 1731," *New York Historical Society Quarterly*, 57 (1973): 255–59. For estimates of the Northern black population predating these censuses, see U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1957* (Washington, 1960), 756.

⁶ Edgar J. McManus, *Black Bondage in the North* (Syracuse, N.Y., 1973), 42–43; Charles S. Boyer, *Early Forges and Furnaces in New Jersey* (Philadelphia, 1963), 30–31, 149, 166, 194–99, 239; Frances D. Pigeon, "Slavery in New Jersey on the Eve of the Revolution," in Williams C. Wright, ed., *New Jersey in the American Revolution* (rev. ed., Trenton, N.J., 1974), 51–52, 57; Darold D. Wax, "The Demand for Slave Labor in Colonial Pennsylvania," *Pennsylvania History*, 34 (1967): 334–35; and William Binning, *Pennsylvania Iron Manufacture in the Eighteenth Century* (Harrisburg, Pa., 1931), 122–25.

access to slaves. Some, like the Barbadian émigrés in northern New Jersey, had migrated from the sugar islands. Others, particularly those around Narragansett Bay, styled themselves planters in the West Indian manner. They built great houses, bred race horses, and accumulated slaves, sometimes holding twenty or more bondsmen. But, whatever the aspirations of this commercial gentry, the provisioning trade could not support a plantation regime. Most slaves lived on farms (not plantations), worked at a variety of tasks, and never labored in large gangs. No one in the North suggested that agricultural labor could be done only by black people, a common assertion in the sugar islands and the Carolina low-country. In northern New England, the Hudson Valley, and Pennsylvania, the seasonal demands of cereal farming undermined the viability of slavery. For most wheat farmers, as Peter Kalm shrewdly observed, "a Negro or black slave requires too much money at one time," and they relied instead on white indentured servants and free workers to supplement their own labor. Throughout the North's bread basket, even those members of the gentry who could afford the larger capital investment and the concomitant risk that slave ownership entailed generally depended on the labor of indentured servants more than on that of slaves. Fully two-thirds of the bond servants held by the wealthiest farmers in Lancaster and Chester counties, Pennsylvania, were indentured whites rather than chattel blacks. These farmers tended to view their slaves more as status symbols than as agricultural workers. While slaves labored in the fields part of the year, as did nearly everyone, they also spent a large portion of their time working in and around their masters' houses as domestic servants, stable keepers, and gardeners. Significantly, the wills and inventories of Northern slaveholders listed their slaves with other high status objects like clocks and carriages rather than with land or agricultural implements.⁷

The distinct demands of Northern agriculture shaped black life in the countryside. Where the provisioning trade predominated, black men worked as stock minders and herdsman while black women labored as dairy maids as well as domestics of various kinds. The large number of slaves demanded by the provisioning trade and the ready access to horses and mules it allowed placed black companionship within easy reach of most bondsmen. Such was not always true in the cereal region. Living scattered throughout the countryside on the largest farms and working in the house as often as in the field, blacks enjoyed neither the mobility nor the autonomy of slaves employed in the provisioning trade. But, if the demands of Northern agriculture affected black life in different ways, almost all rural blacks lived and worked in close proximity to whites. Slaves

⁷ Kalm, *Peter Kalm's Travels in North America*, ed. and trans. A. B. Benson, 1 (New York, 1937): 205, as quoted in Alan Tully, "Patterns of Slaveholding in Colonial Pennsylvania: Chester and Lancaster Counties, 1729-1758," *Journal of Social History*, 6 (1973): 286; Lorenzo J. Greene, *The Negro in Colonial New England* (New York, 1942), 103-12; McManus, *Black Bondage in the North*, 40-41; Pigeon, "Slavery in New Jersey," 51; William D. Miller, "The Narragansett Planters," *American Antiquarian Society Proceedings*, 43 (1933): 67-71; Tully, "Patterns of Slaveholding in Colonial Pennsylvania," 284-303; Steven B. Frankt, "Patterns of Slave-Holding in Somerset County, N.J.," seminar paper, 1967, in Special Collections, Rutgers University Library, New Brunswick, N.J.; Wax, "The Demand for Slave Labor in Colonial Pennsylvania," 332-40; and Jerome H. Woods, Jr., "The Negro in Early Pennsylvania: The Lancaster Experience, 1730-1790," in Elinor Miller and Eugene D. Genovese, eds., *Plantation, Town, and County: Essays on the Local History of American Slave Society* (Urbana, Ill., 1974), 447-48.

quickly learned the rudiments of the English language, the Christian religion, the white man's ways. In the North, few rural blacks remained untouched by the larger forces of Euro-American life.

Northern slaves were also disproportionately urban. During the eighteenth century, a fifth to a quarter of the blacks in New York lived in New York City. Portsmouth and Boston contained fully a third of the blacks in New Hampshire and Massachusetts, and nearly half of Rhode Island's black population resided in Newport. Ownership of slaves was almost universal among the urban elite and commonplace among the middling classes as well. On the eve of the Revolution, nearly three-fourths of Boston's wealthiest quartile of propertyholders ranked in the slaveholding class. Fragmentary evidence from earlier in the century suggests that urban slave-ownership had been even more widespread but contracted with the growth of a free working class. Viewed from the top of colonial society, the observation of one visitor that there was "not a house in Boston" that did "not have one or two" slaves might be applied to every Northern city with but slight exaggeration.⁸

Urban slaves generally worked as house servants—cooking, cleaning, tending gardens and stables, and running errands. They lived in back rooms, lofts, closets, and, occasionally, makeshift alley shacks. Under these cramped conditions, few masters held more than one or two slaves. However they might cherish a large retinue of retainers, urban slaveholders rarely had the room to lodge them. Because of the general shortage of space, masters discouraged their slaves from establishing families in the cities. Women with reputations for fecundity found few buyers, and some slaveholders sold their domestics at the first sign of pregnancy. A New York master candidly announced the sale of his cook "because she breeds too fast for her owners to put up with such inconvenience," and others gave away children because they were an unwarranted expense. As a result, black women had few children, and their fertility ratio was generally lower than that of whites. The inability or unwillingness of urban masters to support large households placed a severe strain on black family life.⁹ But it also encouraged masters to allow their slaves to live out, hire their own time, and thereby gain a measure of independence and freedom.

Slave hirelings along with those bondsmen owned by merchants, warehouse

⁸ N. B. Shurtleff et al., eds., *Records of the Governor and Company of Massachusetts Bay in New England (1628–1698)*, 1 (Boston, 1853): 79, as quoted in Carl Bridenbaugh, *Cities in the Wilderness, 1625–1742* (New York, 1938), 49; Rossiter, *A Century of Population Growth*, 149–84; Greene, *The Negro in Colonial New England*, 78, 81–82, 84–88, 92–93; Gary B. Nash, "Slaves and Slaveowners in Colonial Philadelphia," *William and Mary Quarterly*, 3d ser., 30 (1973): 226–52; and Thomas Archdeacon, *New York City, 1664–1710: Conquest and Change* (Ithaca, N.Y., 1976), 46–47.

⁹ New York *Weekly Post-Boy*, May 17, 1756, as quoted in McManus, *Black Bondage in the North*, 38; Carl Bridenbaugh, *Cities in Revolt, 1743–1776* (New York, 1955), 88, 285–86, and *Cities in the Wilderness*, 163, 200–01; Nash, "Slaves and Slaveowners in Colonial Pennsylvania," 243–44; Archdeacon, *New York City*, 89–90; Rossiter, *A Century of Population Growth*, 170–80; Edgar J. McManus, *A History of Slavery in New York* (Syracuse, N.Y., 1966), 44–45, and *Black Bondage in the North*, 37–39; and Wells, *The Population in the British Colonies of America before 1776*, 116–23. The low ratio of women to children may have been the result of high child mortality as well as low fertility. In 1788, J. P. Brissot de Warville observed, "Married Negroes certainly have as many children as whites, but it has been observed that in the cities the death rate of Negro children is higher"; Brissot de Warville, *New Travels in the United States of America, 1788*, ed. Durand Echeverria (Cambridge, Mass., 1964), 232n.

keepers, and ship chandlers kept Northern cities moving. Working outside their masters' houses, these bondsmen found employment as teamsters, wagoners, and stockmen on the docks and drays and in the warehouses and shops that composed the essential core of the mercantile economy. In addition, many slaves labored in the maritime trades not only as sailors on coasting vessels, but also in the rope walks, shipyards, and sail factories that supported the colonial maritime industry. Generally, the importance of these slaves to the growth of Northern cities increased during the eighteenth century. Urban slavery moved steadily away from the household to the docks, warehouses, and shops, as demonstrated by the growing disproportion of slave men in the urban North. Aside from those skills associated with the maritime trades, however, few slaves entered artisan work. Only a handful could be found in the carriage trades that enjoyed higher status and that offered greater opportunity for an independent livelihood and perhaps the chance to buy freedom.¹⁰

In the cities as in the countryside, blacks tended to live and work in close proximity to whites. Northern slaves not only gained first-hand knowledge of their masters' world, but they also rubbed elbows with lower-class whites in taverns, cock fights, and fairs where poor people of varying status mingled.¹¹ If urban life allowed slaves to meet more frequently and enjoy a larger degree of social autonomy than did slavery in the countryside, the cosmopolitan nature of cities speeded the transformation of Africans to Afro-Americans. Acculturation in the cities of the North was a matter of years, not generations.

For many blacks, the process of cultural transformation was well under way before they stepped off the boat. During the first century of American settlement, few blacks arrived in the North directly from Africa. Although American slavers generally originated in the North, few gave priority to Northern ports. The markets to the south were simply too large and too lucrative. Slaves dribbled into the Northern colonies from the West Indies or the mainland South singly, in twos and threes, or by the score but rarely by the boatload. Some came on special order from merchants or farmers with connections to the West Indian trade. Others arrived on consignment, since few Northern merchants specialized in selling slaves. Many of these were the unsalable "refuse" (as traders contemptuously called them) of larger shipments. Northern slaveholders generally disliked these scourgings of the transatlantic trade who, the governor of Massachusetts observed, were "usually the worst servants they have"; they feared that the West Indian re-exports had records of recalcitrance and criminality as well as physical defects. In time, some masters may have come to prefer seasoned slaves because of their knowledge of English, familiarity with work routines, or resistance to New World diseases. But, whatever their preference, Northern colonies could not compete with the wealthier staple-producing colonies for prime African field hands. Before the 1740s, Africans appear to have

¹⁰ Nash, "Slaves and Slaveowners in Colonial Philadelphia," 248-52; Archdeacon, *New York City*, 89-90, esp. 89 n. 16; Greene, *The Negro in Colonial New England*, 111-18; and Bridenbaugh, *Cities in Revolt*, 88, 274, 285-86.

¹¹ Eric Foner, *Tom Paine and Revolutionary America* (New York, 1976), 48-56.

arrived in the North only when a temporary glut made sale impossible in the West Indies and the mainland South. Even then they did not always remain in the North. When conditions in the plantation colonies changed, merchants re-exported them for a quick profit. The absence of direct importation during the early years and the slow, random, haphazard entry of West Indian creoles shaped the development of black culture in the Northern colonies.¹² While the nature of the slave trade prevented the survival of tribal or even shipboard ties that figured so prominently in Afro-American life in the West Indies and the Lower South, it better prepared blacks to take advantage of the special circumstances of their captivity.

Newly arrived blacks, most already experienced in the New World and familiar with their proscribed status, turned Northern bondage to their advantage where they could. They quickly established a stable family life and, unlike newly imported Africans elsewhere on the continent, increased their numbers by natural means during the first generation. By 1708, the governor of Rhode Island observed that the colony's slaves were "supplied by the offspring of those they have already, which increase daily. . . ." The transplanted creoles also seized the opportunities provided by the complex Northern economy, the relatively close ties of master and slave, and, for many, the independence afforded by urban life. In New Amsterdam, for example, the diverse needs of the Dutch mercantile economy induced the West India Company, the largest slaveholder in the colony, to allow its slaves to live out and work on their own in return for a stipulated amount of labor and an annual tribute. "Half-freedom," as this system came to be called, enlarged black opportunities and allowed for the development of a strong black community. When the West India Company refused to make these privileges hereditary, "half-free" slaves organized and protested, demanding that they be allowed to pass their rights to their children. Failing that, New Amsterdam slaves pressed their masters in other ways to elevate their children's status. Some, hearing rumors that baptism meant freedom, tried to gain church membership. A Dutch prelate complained that these blacks "wanted nothing else than to deliver their children from bodily slavery, without striving for piety and Christian virtues." Even after the conquering English abolished "half-freedom" and instituted a more rigorous system of racial servitude, blacks continued to use the leverage gained by their prominent role in the city's economy to set standards of treatment well above those in the plantation colonies. Into the eighteenth century, New York slaves informally enjoyed the rights of an earlier era, including the right to hold property of their own. "The

¹² W. N. Sainsbury *et al.*, eds., *Calendar of State Papers, Colonial Series, 1708-1709*, 110, as quoted in Greene, *The Negro in Colonial New England*, 35; McManus, *Black Bondage in the North*, 18-25, and *Slavery in New York*, 23-39; James G. Lydon, "New York and the Slave Trade, 1700 to 1774," *William and Mary Quarterly*, 3d ser., 35 (1978): 275-79, 381-90; Greene, *The Negro in Colonial New England*, 15-45; and Darold D. Wax, "Negro Imports into Pennsylvania, 1720-1766," *Pennsylvania History*, 32 (1965): 254-87, and "Preferences for Slaves in Colonial America," *Journal of Negro History*, 58 (1973): 374-76, 379-87. So many of the slaves entering the North were re-exports from other parts of the Americas that Philip D. Curtin has not included the North in his calculation of the African population transported to the New World; see *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969), 143.

Customs of this Country," bristled a frustrated New York master to a West Indian friend, "will not allow us to use our Negroes as you do in Barbados."¹³

Throughout the North, the same factors that mitigated the harshest features of bondage in New York strengthened the position of slaves in dealing with their masters. Small holdings, close living conditions, and the absence of gang labor drew masters and slaves together. A visitor to Connecticut noted in disgust that slaveowners were "too Indulgent (especially the farmers) to their Slaves, suffering too great a familiarity from them, permitting them to sit at Table and eat with them (as they say to save time) and into the dish goes the black hoof as freely as the white hand." Slaves used knowledge gained at their masters' tables to press for additional privileges: the right to visit friends, live with their families, or hire their own time. One slaveholder reluctantly cancelled the sale of his slaves because of "an invariable indulgence here to permit Slaves of any kind of worth or Character who must change Masters, to choose those Masters," and he could not persuade his slaves "to leave their Country (if I may call it so), their acquaintances & friends."¹⁴ Such indulgences originated not only in the ability of slaves to manipulate their masters to their own benefit, but also from the confidence of slaveholders in their own hegemony. Surety of white dominance, derived from white numerical superiority, complemented the blacks' understanding of how best to bend bondage to their own advantage and to maximize black opportunities within slavery.

DURING the middle decades of the eighteenth century, the nature of Northern slavery changed dramatically. Growing demand for labor, especially when European wars limited the supply of white indentured servants and when depression sent free workers west in search of new opportunities, increased the importance of slaves in the work force. Between 1732 and 1754, blacks composed fully a third of the immigrants (forced and voluntary) arriving in New York. The new importance of slave labor changed the nature of the slave trade. Merchants who previously took black slaves only on consignment now began to import them directly from Africa, often in large numbers. Before 1741, for example, 70 percent of the slaves arriving in New York originated in the West Indies and other mainland sources and only 30 percent came directly from Africa. After that date, the proportions were reversed. Specializing in the slave trade, African slavers carried many times more slaves than did West Indian traders. Whereas slaves had earlier arrived in small parcels rarely numbering more than a half-

¹³ Governor Samuel Cranston to the Board of Trade, December 5, 1708, in J. R. Bartlett, ed., *Records of the Colony of Rhode Island and Providence Plantations*, 4 (1860): 55, as quoted in Miller, "Narragansett Planters," 68 n. 2; and Cadwallader Colden to Mr. Jordan, March 26, 1717, in *Letters and Papers of Cadwallader Colden*, 1 (New York, 1917): 39, as quoted in Arthur Zilversmit, *The First Emancipation: The Abolition of Negro Slavery in the North* (Chicago, 1967), 22. Joyce D. Goodfriend, "Burghers and Blacks: The Evolution of a Slave Society at New Amsterdam," *New York History*, 59 (1978): 125-44; McManus, *Slavery in New York*, 2-22; and Gerald F. DeJong, "The Dutch Reformed Church and Negro Slavery in Colonial America," *Church History*, 40 (1971): 430.

¹⁴ Sara Kemble Knight, as quoted in Ralph F. Weld, *Slavery in Connecticut* (New Haven, 1935), 8-9; John Watts, *Letterbook of John Watts*, New York Historical Society Collections, no. 61 (New York, 1938), 151; and McManus, *Black Bondage in the North*, *passim*.

dozen, direct shipments from Africa at times now totaled over a hundred and, occasionally, several times that. Slaves increasingly replaced white indentured servants as the chief source of unfree labor not only in the areas that had produced for the provisioning trade, where their pre-eminence had been established earlier in the century, but in the cities as well. In the 1760s, when slave importation into Pennsylvania peaked, blacks composed more than three-quarters of Philadelphia's servant population.¹⁵

Northern whites generally viewed this new wave of slaves as substitutes for indentured labor. White indentured servants had come as young men without families, and slaves were now imported in much the same way. "For this market they must be young, the younger the better if not quite children," declared a New York merchant. "Males are best." As a result, the sex ratio of the black population, which earlier in the century had been roughly balanced, suddenly swung heavily in favor of men. In Massachusetts, black men outnumbered black women nearly two to one. Elsewhere sex ratios of 130 or more became commonplace.¹⁶ Such sexual imbalance and the proscription of interracial marriage made it increasingly difficult for blacks to enjoy normal family lives. As the birth rate slipped, mortality rates soared, especially in the cities where newly arrived blacks appeared to be concentrated. Since most slaves came without any previous exposure to New World diseases, the harsh Northern winters took an ever higher toll. Blacks died by the score; the crude death rate of Philadelphia and Boston blacks in the 1750s and 1760s was well over sixty per thousand, almost double that of whites.¹⁷ In its demographic outline, Northern slavery at mid-century often bore a closer resemblance to the horrors of the West Indies during the height of a sugar boom than to the relatively benign bondage of the earlier years.

Whites easily recovered from this demographic disaster by again switching to European indentured servants and then to free labor as supplies became available, and, as the influx of slaves subsided, black life also regained its balance. But the transformation of Northern slavery had a lasting influence on the development of Afro-American culture. Although the Northern black population remained predominantly Afro-American after nearly a century of slow importation from the West Indies and steady natural increase, the direct entry of Africans into Northern society reoriented black culture.

Even before the redirection of the Northern slave trade, those few Africans in the Northern colonies often stood apart from the creole majority. While Afro-American slaves established precedents and customs, which they then drew upon to improve their condition, Africans tended to stake all to recapture the

¹⁵ Nash, "Slaves and Slaveowners in Colonial Philadelphia," 226-37; Lydon, "New York and the Slave Trade," 387-88; and Darold D. Wax, "Quaker Merchants and the Slave Trade in Colonial Pennsylvania," *Pennsylvania Magazine of History and Biography*, 86 (1962): 145, and "Negro Imports into Pennsylvania," 256-57, 280-87.

¹⁶ Watts, *Letterbook of John Watts*, 31; McManus, *Black Bondage in the North*, 38-39; Wax, "Preferences for Slaves in Colonial America," 400-01; Rossiter, *A Century of Population Growth*, 149-84; and Greene, *The Negro in Colonial New England*, 93-96.

¹⁷ Nash, "Slaves and Slaveowners in Colonial Philadelphia," 232-41, esp. n. 46.

world they had lost. Significantly, Africans, many of whom did not yet speak English and still carried tribal names, composed the majority of the participants in the New York slave insurrection of 1712, even though most of the city's blacks were creoles.¹⁸ The division between Africans and Afro-Americans became more visible as the number of Africans increased after mid-century. Not only did creoles and Africans evince different aspirations, but their life-chances—as reflected in their resistance to disease and their likelihood of establishing a family—also diverged sharply. Greater visibility may have sharpened differences between creoles and Africans, but Africans were too few in number to stand apart for long. Whatever conflicts different life-chances and beliefs created, whites paid such distinctions little heed in incorporating the African minority into their slaveholdings. The propensity of Northern whites to lump blacks together mitigated intraracial differences. Rather than permanently dividing blacks, the entry of Africans into Northern society gave a new direction to Afro-American culture.¹⁹

Newly arrived Africans reawakened Afro-Americans to their African past by providing direct knowledge of West African society. Creole blacks began to combine their African inheritance into their own evolving culture. In some measure, the easy confidence of Northern whites in their own dominance speeded the syncretization of African and creole culture by allowing blacks to act far more openly than slaves in the plantation colonies. Northern blacks incorporated African culture into their own Afro-American culture not only in the common-place and unconscious way that generally characterizes the transit of culture but also with a high degree of consciousness and deliberateness. They designated their churches “African,” and they called themselves “Sons of Africa.”²⁰ They adopted African forms to maximize their freedom, to choose their leaders, and, in general, to give shape to their lives. This new African influence was manifested most fully in Negro election day, a ritual festival of role reversal common throughout West Africa and celebrated openly by blacks in New England and a scattering of places in the Middle Colonies.

The celebration of Negro election day took a variety of forms, but everywhere it was a day of great merrymaking that drew blacks from all over the countryside. “All the various languages of Africa, mixed with broken and ludicrous English, filled the air, accompanied with the music of the fiddle, tambourine, the banjo, [and] drum,” recalled an observer of the festival in Newport. Negro election day culminated with the selection of black kings, governors, and judges. These officials sometimes held symbolic power over the whole community and real power over the black community. While the black governors held court, ad-

¹⁸ Kenneth Scott, “The Slave Insurrection in New York in 1712,” *New York Historical Society Quarterly*, 45 (1961): 43–74, esp. 62–67.

¹⁹ The shortage of African women and a sexual balance among Indians and, to a lesser extent, whites that favored women encouraged black men to marry Indian and, occasionally, white women, especially in New England; Winthrop D. Jordan, “American Chiaroscuro: The Status and Definition of Mulattoes in the British Colonies,” *William and Mary Quarterly*, 3d ser., 19 (1962): 197–98, esp. n. 28.

²⁰ For petitions by blacks, see Robert C. Twombly, “Black Resistance to Slavery in Massachusetts,” in William L. O’Neill, ed., *Insights and Parallels* (Minneapolis, 1973), 13–16; and, for various association names, see Dorothy Porter, ed., *Early Negro Writings, 1760–1837* (Boston, 1971).

judicating minor disputes, the blacks paraded and partied, dressed in their masters' clothes and mounted on their masters' horses. Such role reversal, like similar status inversions in Africa and elsewhere, confirmed rather than challenged the existing order, but it also gave blacks an opportunity to express themselves more fully than the narrow boundaries of slavery ordinarily allowed. Negro election day permitted a seeming release from bondage, and it also provided a mechanism for blacks to recognize and honor their own notables. Most important, it established a framework for the development of black politics. In the places where Negro election day survived into the nineteenth century, its politics shaped the politics within the black community and merged with partisan divisions of American society. Slaves elsewhere in the New World also celebrated this holiday, but whites in the plantation colonies found the implications of role reversal too frightening to allow even symbolically. Northern whites, on the other hand, not only aided election day materially but sometimes joined in themselves. Still, white cooperation was an important but not the crucial element in the rise of Negro election day. Its origin in the 1740s and 1750s suggests how the entry of Africans reoriented Afro-American culture at a formative point in its development.²¹

African acculturation in the Northern colonies at once incorporated blacks into American society and sharpened the memory of their African past and their desire to preserve it. While small numbers and close proximity to whites forced blacks to conform to the forms of the dominant Euro-American culture, the confidence of whites in their own hegemony allowed black slaves a good measure of autonomy. In this context it is not surprising that a black New England sea captain established the first back-to-Africa movement in mainland North America.²²

UNLIKE AFRICAN ACCULTURATION IN THE NORTHERN COLONIES, the transformation of Africans into Afro-Americans in the Carolina and Georgia lowcountry was a slow, halting process whose effects resonated differently within black society. While creolization created a unified Afro-American population in the North, it left lowcountry blacks deeply divided. A minority lived and worked in close proximity to whites in the cities that lined the rice coast, fully conversant with the most cosmopolitan sector of lowland society. A portion of this urban elite, increasingly light-skinned, pressed for further incorporation into white society, confident they could compete as equals. The mass of black people, however, remained physically separated and psychologically estranged from the Anglo-American world and culturally closer to Africa than any other blacks on continental North America.

²¹ Henry Bull, "Memoir of Rhode Island," Newport *Rhode-Island Republican*, April 19, 1837, as quoted in William D. Pierson, "Afro-American Culture in Eighteenth-Century New England" (Ph.D. dissertation, Indiana University, 1975), 181; Joseph P. Reidy, "'Negro Election Day' and Black Community Life in New England, 1750-1860," *Marxist Perspectives*, 1 (1978): 102-17; Alice M. Earle, *Colonial Days in Old New York* (5th ed., New York, 1922); Woods, "The Negro in Early Pennsylvania," 451; and Pierson, "Afro-American Culture in Eighteenth-Century New England," 181-313.

²² Peter Williams, *A Discourse, Delivered in the Death of Capt. Paul Cuffee* (New York, 1817).

The sharp division was not immediately apparent. At first it seemed that African acculturation in the Lower South would follow the Northern pattern. The first blacks arrived in the lowcountry in small groups from the West Indies. Often they accompanied their owners and, like them, frequently immigrated in small family groups. Many had already spent considerable time on the sugar islands, and some had doubtless been born there. Most spoke English, understood European customs and manners, and, as their language skills and family ties suggest, had made the difficult adjustment to the conditions of black life in the New World.

As in the Northern colonies, whites dominated the population of the pioneer Carolina settlement. Until the end of the seventeenth century, they composed better than two-thirds of the settlers. During this period and into the first years of the eighteenth century, most white slaveholders engaged in mixed farming and stock raising for export to the West Indian islands where they had originated. Generally, they lived on small farms, held few slaves, and worked closely with their bond servants. Even when they hated and feared blacks and yearned for the prerogatives of West Indian slave masters, the demands of the primitive, labor-scarce economy frequently placed master and slave face-to-face on opposite sides of a sawbuck.²³ Such direct, equalitarian confrontations tempered white domination and curbed slavery's harshest features.

White dependence on blacks to defend their valuable lowland beachhead reinforced this "sawbuck equality." The threat of invasion by the Spanish and French to the south and Indians to the west hung ominously over the lowcountry during its formative years. To bolster colonial defenses, officials not only drafted slaves in time of war but also regularly enlisted them into the militia. In 1710 Thomas Nairne, a knowledgeable Carolina Indian agent, observed that "enrolled in our Militia [are] a considerable Number of active, able, Negro Slaves; and Law gives every one of those his freedom, who in Time of an Invasion kills an Enemy." Between the settlement of the Carolinas and the conclusion of the Yamasee War almost fifty years later, black soldiers helped fend off every military threat to the colony. Although only a handful of slaves won their freedom through military service, the continued presence of armed, militarily experienced slaves weighed heavily on whites. During the Yamasee War, when the governor of Virginia demanded one Negro woman in return for each Virginia soldier sent to defend South Carolina, the beleaguered Carolinians rejected the offer, observing that it was "impracticable to Send Negro Women in their Roomes by reason of the Discontent such Usage would have given their husbands to have their wives taken from them which might have occasioned a Revolt."²⁴

²³ Peter H. Wood, *Black Majority: Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), 13-24, 94-97. The image is derived from an account of a French refugee living near the Santee River who reported in 1697 that "he worked many days with a Negro man at the Whip saw"; Alexander S. Salley, ed., "Journal of General Peter Horry," *South Carolina Historical Magazine*, 38 (1937): 51-52, as quoted in *ibid.*, 97.

²⁴ Memorial of Joseph Boone and Richard Beresford to the Lord Commissioners of Trade and Plantations, December 6, 1716, Public Record Office, London, as quoted in Clarence L. Ver Steeg, *Origins of a Southern Mosaic: Studies of Early Carolina and Georgia* (Athens, Ga., 1975), 106; Wood, *Black Majority: Negroes in Colonial*

The unsettled conditions that made the lowcountry vulnerable to external enemies strengthened the slave's hand in other ways. Confronted by an overbearing master or a particularly onerous assignment, many blacks took to the woods. Truancy was an easy alternative in the thinly settled, heavily forested lowcountry. Forest dangers generally sent truant slaves back to their owners, but the possibility of another flight induced slaveholders to accept them with few questions asked. Some bondsmen, however, took advantage of these circumstances to escape permanently. Maroon colonies existed throughout the lowland swamps and into the backcountry. Maroons lived a hard life, perhaps more difficult than slaves, and few blacks chose to join these outlaw bands. But the ease of escape and the existence of a maroon alternative made masters chary about abusing their slaves.²⁵

The transplanted African's intimate knowledge of the subtropical lowland environment—especially when compared to the Englishman's dense ignorance—magnified white dependence on blacks and enlarged black opportunities within the slave regime. Since the geography, climate, and topography of the lowcountry more closely resembled the West African than the English countryside, African not European technology and agronomy often guided lowland development. From the first, whites depended on blacks to identify useful flora and fauna and to define the appropriate methods of production. Blacks, adapting African techniques to the circumstances of the Carolina wilderness, shaped the lowland cattle industry and played a central role in the introduction and development of the region's leading staple. In short, transplanted Englishmen learned as much or more from transplanted Africans as did the former Africans from them.²⁶ While whites eventually appropriated this knowledge and turned it against black people to rivet tighter the bonds of servitude, white dependence on African know-how operated during those first years to place blacks in managerial as well as menial positions and thereby permitted blacks to gain a larger share of the fruits of the new land than whites might otherwise allow. In such circumstances, white domination made itself felt, but both whites and blacks incorporated much of West African culture into their new way of life.

The structure of the fledgling lowland economy and the demands of stock raising, with deerskins as the dominant "crop" during the initial years of settlement, allowed blacks to stretch white military and economic dependence into generous grants of autonomy. On the small farms and isolated cowpens (hardly plantations by even the most latitudinous definition), rude frontier conditions permitted only perfunctory supervision and the most elementary division of labor. Most units were simply too small to employ overseers, single out specialists, or benefit from the economies of gang labor. White, red, and black laborers of varying legal status worked shoulder to shoulder, participating in the dullest

South Carolina, 124–30; Ver Steeg, *Origins of a Southern Mosaic*, 105–07; and Verner W. Crane, *The Southern Frontier, 1670–1732* (Durham, N.C., 1928), 162–81.

²⁵ John D. Duncan, "Servitude and Slavery in Colonial South Carolina, 1670–1776" (Ph.D. dissertation, Emory University, 1971), 587–601; and Herbert Aptheker, "Maroons within the Present Limits of the United States," *Journal of Negro History*, 24 (1939): 167–84.

²⁶ Wood, *Black Majority: Negroes in Colonial South Carolina*, 35–62, 119–30.

drudgery as well as the most sophisticated undertakings. Rather than skilled artisans or prime field hands, most blacks could best be characterized as jacks-of-all-trades. Since cattle roamed freely through the woods until fattened for market, moreover, black cowboys—suggestively called “cattle chasers”—moved with equal freedom through the countryside, gaining full familiarity with the terrain.²⁷ The autonomy of the isolated cowpen and the freedom of movement stock raising allowed made a mockery of the total dominance that chattel bondage implied. Slaves set the pace of work, defined standards of workmanship, and divided labor among themselves, doubtless leaving a good measure of time for their own use. The insistence of many hard-pressed frontier slaveowners that their slaves raise their own provisions legitimated this autonomy. By law, slaves had Sunday to themselves. Time allowed for gardening, hunting, and fishing both affirmed slave independence and supplemented the slave diet. It also enabled some industrious blacks to produce a small surplus and to participate in the colony’s internal economy, establishing an important precedent for black life in the lowcountry.²⁸

Such independence burdened whites. They complained bitterly and frequently about blacks traveling unsupervised through the countryside, congregating in the woods, and visiting Charles Town to carouse, conspire, or worse. Yet knowledge of the countryside and a willingness to take the initiative in hunting down cattle or standing up to Spaniards were precisely the characteristics that whites valued in their slaves. They complained but they accepted. Indeed, to resolve internal disputes within their own community, whites sometimes promoted black participation in the affairs of the colony far beyond the bounds later permitted slaves or even black freemen. “For this last election,” grumbled several petitioners in 1706, “Jews, Strangers, Sailors, Servants, Negroes, & almost every French Man in Craven & Berkly County came down to elect, & their votes were taken.”²⁹ Such breaches of what became an iron law of Southern racial policy suggest how the circumstances of the pioneer lowcountry life shrank the social as well as the cultural distance between transplanted Africans and the *mélange* of European settlers. During the first generations of settlement, Afro-American and Anglo-American culture and society developed along parallel lines with a large degree of overlap.

²⁷ *Ibid.*, 28–34; Converse D. Clowse, *Economic Beginnings of Colonial South Carolina, 1670–1730* (Columbia, S.C., 1971), 61; Crane, *The Southern Frontier, 1670–1732*, 91, 120, 163, 184–85; Ver Steeg *Origins of a Southern Mosaic*, 114–16; Gary S. Dunbar, “Colonial Carolina Cowpens,” *Agricultural History*, 35 (1961): 125–30; and David L. Coon, “The Development of Market Agriculture in South Carolina, 1670–1785” (Ph.D. dissertation, University of Illinois, Urbana-Champaign, 1972), 113–14, 134–37. Georgia developed later than South Carolina; a description of an isolated cowpen in the Georgia countryside in 1765 may, therefore, suggest practices of an earlier era in South Carolina. See Harold E. Davis, *The Fledgling Province: Social and Cultural Life in Colonial Georgia, 1733–1776* (Chapel Hill, N.C., 1976), 67–68.

²⁸ Frank J. Klingberg, *An Appraisal of the Negro in Colonial South Carolina* (Washington, 1941), 6–7; Klaus G. Leowald, Beverly Starika, and Paul S. Taylor, trans. and eds., “Johann Martin Bolzious Answers a Questionnaire on Carolina and Georgia,” *William and Mary Quarterly*, 3d ser., 14 (1957): 235–36, 256; Thomas Cooper and David J. McCord, comps., *The Statutes at Large of South Carolina*, 10 vols. (Columbia, S.C., 1836–41), 7: 404; and Wood, *Black Majority: Negroes in Colonial South Carolina*, 62. For black participation in the internal economy of the sugar islands, see Sidney W. Mintz, *Caribbean Transformations* (Chicago, 1974), esp. chap. 7.

²⁹ “The Representation and Address of Several Members of This Present Assembly,” in William James Rivers, *A Sketch of the History of South Carolina* (Charleston, S.C., 1856), 459, as quoted in Ver Steeg, *Origins of a Southern Mosaic*, 38 (italics removed); and Wood, *Black Majority: Negroes in Colonial South Carolina*, 102–03.

If the distinction between white and black culture remained small in the lowcountry, so too did differences within black society. The absence of direct importation of African slaves prevented the emergence of African-creole differences; and, since few blacks gained their liberty during those years, differences in status within the black community were almost nonexistent. The small radius of settlement and the ease of water transportation, moreover, placed most blacks within easy reach of Charles Town. A "city" of several dozen rude buildings where the colonial legislature met in a tavern could hardly have impressed slaves as radically different from their own primitive quarters. Town slaves, for their part, doubtless had first-hand familiarity with farm work as few masters could afford the luxury of placing their slaves in livery.³⁰

Thus, during the first years of settlement, black life in the lowcountry, like black life in the North, evolved toward a unified Afro-American culture. Although their numbers combined with other circumstances to allow Carolina blacks a larger role in shaping their culture than that enjoyed by blacks in the North, there remained striking similarities in the early development of Afro-American life in both regions. During the last few years of the seventeenth century, however, changes in economy and society undermined these commonalities and set the development of lowcountry Afro-American life on a distinctive course.

THE discovery of exportable staples, first naval stores and then rice and indigo, transformed the lowcountry as surely as the sugar revolution transformed the West Indies. Under the pressure of the riches that staple production provided, planters banished the white yeomanry to the hinterland, consolidated small farms into large plantations, and carved new plantations out of the malaria-ridden swamps. Before long, black slaves began pouring into the region and, sometime during the first decade of the eighteenth century, white numerical superiority gave way to the lowcountry's distinguishing demographic characteristic: the black majority.

Black numerical dominance grew rapidly during the eighteenth century. By the 1720s, blacks outnumbered whites by more than two to one in South Carolina. In the heavily settled plantation parishes surrounding Charles Town, blacks enjoyed a three to one majority. That margin grew steadily until the disruptions of the Revolutionary era, but it again increased thereafter. Georgia, where metropolitan policies reined planter ambition, remained slaveless until mid-century. Once restrictions on slavery were removed, planters imported blacks in large numbers, giving lowland Georgia counties considerable black majorities.³¹

Direct importation of slaves from Africa provided the impetus to the growth

³⁰ Wood, *Black Majority: Negroes in Colonial South Carolina*, 99–103, 157, 159.

³¹ Peter H. Wood, "More like a Negro Country": Demographic Patterns in Colonial South Carolina, 1670–1740," in Stanley L. Engerman and Eugene D. Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, 1975), 131–45; Julian J. Petty, *The Growth and Distribution of Population in South Carolina* (Columbia, S.C., 1943), 15–58, 220–27; Bureau of the Census, *Historical Statistics of the United States*, 756; and *Returns of the Whole Number of Persons within the . . . United States [1790]* (Philadelphia, 1791).

of the black majority. Some West Indian Afro-Americans continued to enter the lowcountry, but they shrank to a small fraction of the whole.³² As African importation increased, Charles Town took its place as the largest mainland slave mart and the center of the lowland slave trade. Almost all of the slaves in Carolina and later in Georgia—indeed, fully 40 percent of all pre-Revolutionary black arrivals in mainland North America—entered at Charles Town. The enormous number of slaves allowed slave masters a wide range of choices. Lowcountry planters developed preferences far beyond the usual demands for healthy adult and adolescent males and concerned themselves with the regional and tribal origins of their purchases. Some planters may have based their choices on long experience and a considered understanding of the physical and social character of various African nations. But, for the most part, these preferences were shallow ethnic stereotypes. Coromantees revolted; Angolans ran away; Iboes destroyed themselves. At other times, lowland planters apparently preferred just those slaves they did not get, perhaps because all Africans made unsatisfactory slaves and the unobtainable ones looked better at a distance. Although lowcountry slave masters desired Gambian people above all others, Angolans composed a far larger proportion of the African arrivals. But, however confused or mistaken in their beliefs, planters held them firmly and, in some measure, put them into practice. “Gold Coast and Gambia’s are the best, next to them the Windward Coast are prefer’d to Angola’s,” observed a Charles Town merchant in describing the most salable mixture. “There must not be a Callabar amongst them.”³³ Planter preferences informed lowcountry slave traders and, to a considerable degree, determined the tribal origins of lowland blacks.

Whatever their origins, rice cultivation shaped the destiny of African people arriving at Charles Town. Although the production of pitch and tar played a pivotal role in the early development of the staple-based economy in South Carolina, rice quickly became the dominant plantation crop. Rice cultivation evolved slowly during the late seventeenth and early eighteenth centuries as planters, aided by knowledgeable blacks, mastered the complex techniques necessary for commercial production. During the first half of the eighteenth century, rice culture was limited to the inland swamps, where slave-built dikes controlled the irrigation of low-lying rice fields. But by mid-century planters had discovered how to regulate the tidal floods to irrigate and drain their fields. Rice production moved to the tidal swamps that lined the region’s many rivers and expanded greatly. By the beginning of the nineteenth century, the rice coast stretched from Cape Fear in North Carolina to the Satilla River in Georgia.³⁴ Throughout the lowcountry, rice was king.

³² W. Robert Higgins, “Charleston: Terminus and Entrepôt of the Colonial Slave Trade,” in Martin L. Kilson and Robert I. Rotberg, eds., *The African Diaspora* (Cambridge, Mass., 1976), 115.

³³ Wood, *Black Majority: Negroes in Colonial South Carolina*, xiv, and “‘More like a Negro Country,’” 149–54; Higgins, “Charleston: Terminus and Entrepôt of the Colonial Slave Trade,” 118–27; Wax, “Preferences for Slaves in Colonial America,” 388–99; Curtin, *The Atlantic Slave Trade*, 143, 156–57; and Henry Laurens, *The Papers of Henry Laurens*, ed. Philip M. Hamer, George C. Rogers, Jr., and David R. Chesnutt, 7 vols. (Columbia, S.C., 1970–), 1: 294–95. For a continuing discussion of slave preferences in the lowcountry, see Laurens, *Papers of Henry Laurens*, esp. vols. 1–3.

³⁴ Clowse, *Economic Beginnings of Colonial South Carolina*, 122–33, 167–71, 220–21, 231–35, 256–58; Wood, *Black*

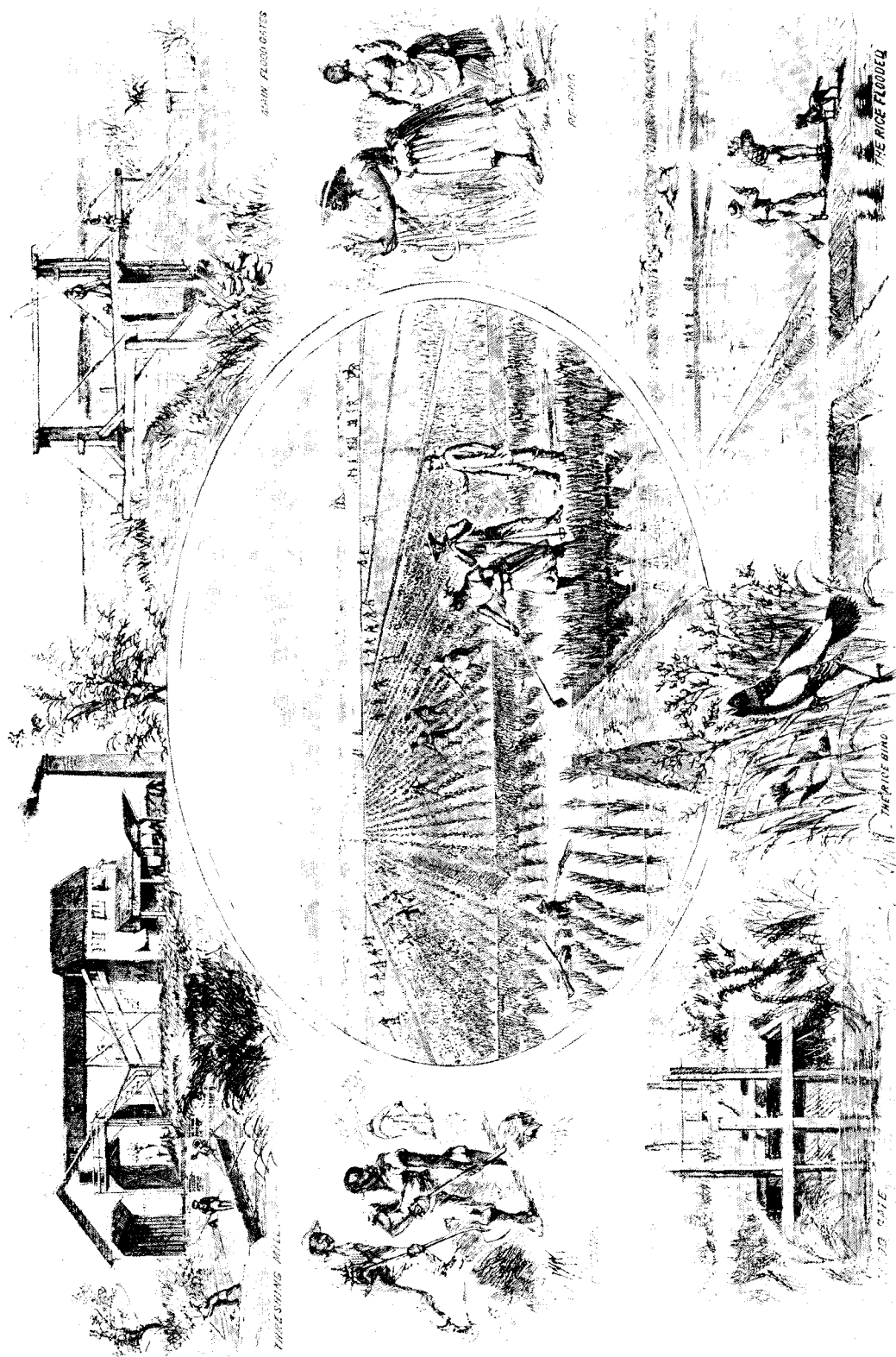


Figure 1: Mid-nineteenth-century representation of the processes of rice production. With the exception of the steam mill in the upper left-hand corner, the processes depicted here had not changed from the colonial era. Photograph taken from *Harper's Weekly*, January 5, 1867.

The relatively mild slave regime of the pioneer years disappeared as rice cultivation expanded. Slaves increasingly lived in large units, and they worked in field gangs rather than at a variety of tasks. The strict requirements of rice production set the course of their work. And rice was a hard master. For a large portion of the year, slaves labored knee deep in brackish muck under the hot tropical sun; and, even after the fields were drained, the crops laid-by, and the grain threshed, there were canals to clear and dams to repair. By mid-century planters had also begun to grow indigo on the upland sections of their estates. Indigo complemented rice in its seasonal requirements, and it made even heavier labor demands.³⁵ The ready availability of African imports compounded the new harsh realities of plantation slavery by cheapening black life in the eyes of many masters. As long as the slave trade remained open, they skimmed on food, clothing, and medical attention for their slaves, knowing full well that substitutes could be easily had. With the planters' reliance on male African imports, slaves found it increasingly difficult to establish and maintain a normal family life. Brutal working conditions, the disease-ridden, lowland environment, and the open slave trade made for a deadly combination. Slave birth rates fell steadily during the middle years of the eighteenth century and mortality rates rose sharply. Between 1730 and 1760, deaths outnumbered births among blacks and only African importation allowed for continued population growth. Not until the eve of the Revolution did the black population begin again to reproduce naturally.³⁶

As the lowcountry plantation system took shape, the great slave masters retreated to the cities of the region; their evacuation of the countryside was but another manifestation of the growing social and cultural distance between them and their slaves. The streets of Charles Town, and, later, of Beaufort, Georgetown, Savannah, Darien, and Wilmington sprouted great new mansions as planters fled the malarial lowlands and the black majority. By the 1740s, urban life in the lowcountry had become attractive enough that men who made their fortunes in rice and slaves no longer returned home to England in the West Indian tradition. Instead, through intermarriage and business connections, they began to weave their disparate social relations into a close-knit ruling class, whose self-consciousness and pride of place became legendary. Charles Town, as the capital of this new elite, grew rapidly. Between 1720 and 1740 its population

Majority: Negroes in Colonial South Carolina, 35-62; Lewis C. Gray, *History of Agriculture in the Southern United States to 1860*, 2 vols. (Washington, 1933), 1: 277-89; James M. Clifton, "Golden Grains of White: Rice Planting on the Lower Cape Fear," *North Carolina Historical Review*, 50 (1973): 368-78; Douglas C. Wilms, "The Development of Rice Culture in 18th-Century Georgia," *Southeastern Geographer*, 12 (1972): 45-57; and Coon, "Market Agriculture in South Carolina," 126-27, 168-69, 178-86, 215-68. For the importance of naval stores in the transformation, see Ver Steeg, *Origins of a Southern Mosaic*, 117-32.

³⁵ For excellent descriptions of the process of rice growing and its changing technology, see David Doar, *Rice and Rice Planting in the Carolina Low Country* (Charleston, S.C., 1936), 7-41; and Gray, *Agriculture in the Southern United States*, 1: 290-97.

³⁶ Wood, "'More like a Negro Country,'" 153-64; and Philip D. Morgan, "Afro-American Cultural Change: The Case of Colonial South Carolina Slaves," paper presented at the annual meeting of the Organization of American Historians, held in New Orleans, April 1979, 3-6, esp. tables 1, 4, 7. In the 1760s, as blacks began to increase naturally, slaveholders began to show some concern for their slaves' family life; see Laurens, *Papers of Henry Laurens*, 4: 595-96, 625, 5: 370.

doubled, and it nearly doubled again by the eve of the Revolution to stand at about twelve thousand. With its many fine houses, its great churches, its shops packed with luxury goods, Charles Town's prosperity bespoke the maturation of the lowland plantation system and the rise of the planter class.³⁷

Planters, ensconced in their new urban mansions, their pockets lined with the riches rice produced, ruled their lowcountry domains through a long chain of command: stewards located in the smaller rice ports, overseers stationed near or on their plantations, and plantation-based black drivers. But their removal from the plantation did not breed the callous indifference of West Indian absenteeism. For one thing, they were no more than a day's boat ride away from their estates. Generally, they resided on their plantations during the non-malarial season. Their physical removal from the direct supervision of slave labor and the leisure their urban residences afforded appear to have sharpened their concern for "their people" and bred a paternalist ideology that at once legitimated their rule and informed all social relations.³⁸

The lowcountry plantation system with its urban centers, its black majority, its dependence on "salt-water" slaves transformed black culture and society just as it reshaped the white world. The unified Afro-American culture and society that had evolved during the pioneer years disappeared as rice cultivation spread. In its place a sharp division developed between an increasingly urban creole and a plantation-based African population. The growth of plantation slavery not only set blacks further apart from whites, it also sharply divided blacks.

One branch of black society took shape within the bounds of the region's cities and towns. If planters lived removed from most slaves, they maintained close, intimate relations with some. The masters' great wealth, transient life, and seasonal urban residence placed them in close contact with house servants who kept their estates, boatmen who carried messages and supplies back and forth to their plantations, and urban artisans who made city life not only possible but comfortable. In addition, coastal cities needed large numbers of workers to transport and process the plantation staples, to serve the hundreds of ships that annually visited the lowcountry, and to satisfy the planters' newly acquired taste for luxury goods. Blacks did most of this work. Throughout the eighteenth century they composed more than half the population of Charles Town and other lowcountry ports. Probably nothing arrived or left these cities without some black handling it. Black artisans also played a large role in urban life. Master craftsmen employed them in every variety of work. A visitor to Charles Town

³⁷ George C. Rogers, Jr., *Charleston in the Age of the Pinckneys* (Norman, Okla., 1969); Carl Bridenbaugh, *Myths and Realities* (Baton Rouge, 1952), 59–60, 76–94, and *Cities in Revolt*, 216; and Frederick P. Bowes, *The Culture of Early Charleston* (Chapel Hill, N.C., 1942).

³⁸ Eugene D. Genovese has not made either regional or temporal distinctions in the development of Southern ideology but has leaned heavily on South Carolina for his understanding of Southern paternalism; see his *Roll, Jordan, Roll*, 1–113. For the interplay of quasi-absenteeism and planter ideology in the nineteenth century, see William W. Freehling, *Prelude to Civil War: The Nullification Controversy in South Carolina, 1813–1836* (New York, 1966), 65–70; and Michael P. Johnson, "Planters and Patriarchy: A Family History of Planter Ideology, Charleston, South Carolina," *Journal of Southern History* (forthcoming). The degree of absenteeism and its effect on social relations between planters and slaves has yet to be explored.

found that even barbers “are supported in idleness & ease by their negroes . . . ; & in fact many of the mechaniks bear nothing more of their trade than the name.” Although most black artisans labored along the waterfront as shipwrights, ropemakers, and coopers, lowcountry blacks—unlike blacks in Northern cities—also entered the higher trades, working as gold beaters, silversmiths, and cabinetmakers. In addition, black women gained control over much of the marketing in the lowcountry ports, mediating between slave-grown produce in the countryside and urban consumption. White tradesmen and journeymen periodically protested against slave competition, but planters, master craftsmen, and urban consumers who benefited from black labor and services easily brushed aside these objections.³⁹

Mobile, often skilled, and occasionally literate, urban slaves understood the white world. They used their knowledge to improve their position within lowcountry society even while the condition of the mass of black people deteriorated in the wake of the rice revolution. Many urban creoles not only retained the independence of the earlier years but enlarged upon it. They hired their own time, earned wages from “overwork,” kept market stalls, and sometimes even opened shops. Some lived apart from their masters and rented houses of their own, paying their owners a portion of their earnings in return for *de facto* freedom. Such liberty enabled a few black people to keep their families intact and perhaps even accumulate property for themselves. The small black communities that developed below the Bluff in Savannah and in Charles Town’s Neck confirm the growing independence of urban creoles.⁴⁰

The incongruous prosperity of urban bondsmen jarred whites. By hiring their own time, living apart from their masters, and controlling their own family life, these blacks forcibly and visibly claimed the white man’s privileges. Perhaps no aspect of their behavior was as obvious and, hence, as galling as their elaborate dress. While plantation slaves—men and women—worked stripped to the waist wearing no more than loin cloths (thereby confirming the white man’s image of savagery), urban slaves appropriated their masters’ taste for fine clothes and often the clothes themselves. Lowcountry legislators enacted various sumptuary regulations to restrain the slaves’ penchant for dressing above their station. The

³⁹ Joseph W. Barnwell, ed., “The Diary of Timothy Ford,” *South Carolina Historical Magazine*, 13 (1914): 142; Alexander Hewatt, *An Historical Account of the Rise and Progress of the Colonies of South Carolina and Georgia*, 2 (London, 1779): 97; Alan Candler, ed., *The Colonial Records of the State of Georgia*, 18 (Atlanta, 1912): 277–82; Charles S. Henry, comp., *A Digest of All the Ordinances of Savannah* (Savannah, Ga., 1854), 94–97; Petition from Charleston Carpenters and Bricklayers, 1783, and Petition from Charleston Coopers, 1793, Legislative Papers, South Carolina Department of Archives and History, Columbia; Cooper and McCord, *Statutes at Large of South Carolina*, 2: 22–23, 7: 385–87, 9: 692–97; Donald R. Lennon and Ida B. Kellam, eds., *The Wilmington Town Book, 1743–1778* (Raleigh, N.C., 1973), 163–66; Petition from Newberne, 1785, North Carolina Legislative Papers, North Carolina State Archives, Raleigh; Carl Bridenbaugh, *Colonial Craftsmen* (New York, 1950) 139–41, and *Cities in Revolt*, 88–89, 244, 274, 285–86; Leila Sellers, *Charleston Business on the Eve of the American Revolution* (Chapel Hill, N.C., 1934), 99–108; Duncan, “Servitude and Slavery in Colonial South Carolina,” 439–46; and Kenneth Coleman, *Colonial Georgia, A History* (New York, 1976), 229–30.

⁴⁰ Candler, *Colonial Records of the State of Georgia*, 23–30, 252–62; Henry, *Ordinances of Savannah*, 95–97; Alexander Edwards, comp., *Ordinances of the City Council of Charleston* (Charleston, S.C., 1802), 65–68; Cooper and McCord, *Statutes at Large of South Carolina*, 7: 363, 380–81, 393; Lennon and Kellam, *The Wilmington Town Book*, xxx–xxxi, 165–68, 204–05; Duncan, “Servitude and Slavery in Colonial South Carolina,” 467–69, 481–84; and Sellers, *Charleston Business on the Eve of the American Revolution*, 99–102, 106–08.

South Carolina Assembly once even considered prohibiting masters from giving their old clothes to their slaves. But hand-me-downs were clearly not the problem as long as slaves earned wages and had easy access to the urban marketplace. Frustrated by the realities of urban slavery, lawmakers passed and re-passed the old regulations to little effect. On the eve of the Revolution, a Charles Town Grand Jury continued to bemoan the fact that the "Law for preventing the excessive and costly Apparel of Negroes and other Slaves in this province (especially in *Charles Town*) [was] not being put into Force."⁴¹

Most of these privileged bondsmen appear to have been creoles with long experience in the New World. Although some Africans entered urban society, the language skills and the mastery of the complex interpersonal relations needed in the cities gave creoles a clear advantage over Africans in securing elevated positions within the growing urban enclaves. To be sure, their special status was far from "equal." No matter how essential their function or intimate their interaction, their relations with whites no longer smacked of the earlier "sawbuck equality." Instead, these relations might better be characterized as paternal, sometimes literally so.

Increasingly during the eighteenth century, blacks gained privileged positions within lowcountry society as a result of intimate, usually sexual, relations with white slave masters. Like slaveholders everywhere, lowland planters assumed that sexual access to slave women was simply another of the master's prerogatives. Perhaps because their origin was West Indian or perhaps because their dual residence separated them from their white wives part of the year, white men established sexual liaisons with black women frequently and openly. Some white men and black women formed stable, long-lasting unions, legitimate in everything but law. More often than other slaveholders on continental British North America, lowcountry planters recognized and provided for their mulatto offspring, and, occasionally, extended legal freedom. South Carolina's small free Negro population, almost totally confined to Charles Town, was largely the product of such relations. Light-skinned people of color enjoyed special standing in the lowcountry ports, as they did in the West Indies, and whites occasionally looked the other way when such creoles passed into the dominant caste. But even when the planters did not grant legal freedom, they usually assured the elevated standing of their mulatto scions by training them for artisan trades or placing them in household positions. If the countryside was "blackened" by African imports, Charles Town and the other lowcountry ports exhibited a *mélange* of "colored" peoples.⁴²

While one branch of black society stood so close to whites that its members

⁴¹ *South Carolina Gazette*, May 24, 1773, as quoted in Duncan, "Servitude and Slavery in Colonial South Carolina," 234; Leowald, *et al.*, "Bolzius Answers a Questionnaire on Carolina and Georgia," 236; Cooper and McCord, *Statutes at Large of South Carolina*, 7: 396-412; and Duncan, "Servitude and Slavery in Colonial South Carolina," 233-37.

⁴² Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro* (Chapel Hill, N.C., 1968), 144-50, 167-78, and "American Chiaroscuro: The Status and Definition of Mulattoes in the British Colonies," 186-200; Wood, *Black Majority: Negroes in Colonial South Carolina*, 100-03; and General Tax, Receipts and Payments, 1761-69, Records of the Public Treasurers of South Carolina, South Carolina Department of Archives and History, Columbia (I am grateful to Peter H. Wood for telling me about these records). A sample of manumissions taken from the South Carolina records between 1729 and 1776 indicates that two-thirds of the slaves

sometimes disappeared into the white population, most plantation slaves remained alienated from the world of their masters, physically and culturally. Living in large units often numbering in the hundreds on plantations that they had carved out of the malarial swamps and working under the direction of black drivers, the black majority gained only fleeting knowledge of Anglo-American culture. What they knew did not encourage them to learn more. Instead, they strove to widen the distance between themselves and their captors. In doing so, they too built upon the large degree of autonomy black people had earlier enjoyed.

In the pioneer period, many masters required slaves to raise their own provisions. Slaves regularly kept small gardens and tended barnyard fowl to maintain themselves, and they often marketed their surplus. Blacks kept these prerogatives with the development of the plantation system. In fact, the growth of lowcountry towns, the increasing specialization in staple production, and the comparative absence of nonslaveholding whites enlarged the market for slave-grown produce. Planters, of course, disliked the independence truck gardening afforded plantation blacks and the tendency of slaves to confuse their owners' produce with their own, but the ease of water transportation and the absence of white supervision made it difficult to prevent.

To keep their slaves on the plantation, some planters traded directly with their bondsmen, bartering manufactured goods for slave produce. Henry Laurens, a planter who described himself as a "factor" for his slaves, exchanged some "very gay Wastcoats which some of the Negro Men may want" for grain at "10 Bushels per Wastcoat." Later, learning that a plantation under his supervision was short of provisions, he authorized the overseer "to purchase of your own Negroes all that you know Lawfully belongs to themselves at the lowest price they will sell it for." As Laurens's notation suggests, planters found benefits in slave participation in the lowcountry's internal economy, but the small profits gained by bartering with their bondsmen only strengthened the slaves' customary right to their garden and barnyard fowl. Early in the nineteenth century, when Charles C. Pinckney decided to produce his own provisions, he purchased breeding stock from his slaves. By the Civil War, lowland slaves controlled considerable personal property—flocks of ducks, pigs, milch cows, and occasionally horses—often the product of stock that had been in their families for generations.⁴³ For the most part, slave propertyholding remained small during the eighteenth century. But it helped insulate plantation blacks from the harsh con-

freed were female and one-third of the slaves freed were mulattoes at a time when the slave population of South Carolina was disproportionately male and black; Duncan, "Servitude and Slavery in Colonial South Carolina," 395-98.

⁴³ Laurens, *Papers of Henry Laurens*, 4: 616, 5: 20, 41; C. C. Pinckney, *Plantation Journal*, 1812, and George Lucas to Charles Pinckney, January 30, 1745/46, Manuscript Division, Library of Congress, Washington, D.C.; Entries in Memo Book "per self" and "Negro Esquire per self," Cameron Family Papers, Southern Historical Collection, University of North Carolina, Chapel Hill; Charles Town Grand Jury Presentment, January 1772, South Carolina Department of Archives and History; and Depositions from Liberty County, Georgia, Southern Claims Commission, Third Auditor, General Accounting Office, RG 217, National Archives, Washington, D.C. A similar division of labor between master and slave has been found in various nineteenth-century African slave societies. Whether these similar patterns have a common root or are the product of independent development is a subject for future research. See Paul O. Lovejoy, "The Characteristics of Plantations in the Nineteenth-Century Sokoto Caliphate (Islamic West Africa)," *AHR*, 84 (1979): 1283-84. Also see footnote 19, above.

ditions of primitive rice production and provided social distance from their masters' domination.

The task system, a mode of work organization peculiar to the lowcountry, further strengthened black autonomy. Under the task system, a slave's daily routine was sharply defined: so many rows of rice to be sowed, so much grain to be threshed, or so many lines of canal to be cleared. Such a precise definition of work suggests that city-bound planters found it almost impossible to keep their slaves in the fields from sunup to sundown. With little direct white supervision, slaves and their black foremen conspired to preserve a large portion of the day for their own use, while meeting their masters' minimum work requirements. Struggle over the definition of a task doubtless continued throughout the formative years of the lowcountry plantation system and after, but by the end of the century certain lines had been drawn. Slaves generally left the field sometime in the early afternoon, a practice that protected them from the harsh afternoon sun and allowed them time to tend their own gardens and stock.⁴⁴ Like participation in the lowcountry's internal economy, the task system provided slaves with a large measure of control over their own lives.

The autonomy generated by both the task system and truck gardening provided the material basis for lowland black culture. Within the confines of the overwhelmingly black countryside, African culture survived well. The continual arrival of Africans into the lowcountry renewed and refreshed slave knowledge of West African life. In such a setting blacks could hardly lose their past. The distinctive pattern of the lowland slave trade, moreover, heightened the impact of the newly arrived Africans on the evolution of black culture. While slaves dribbled into the North through a multiplicity of ports, they poured into the lowcountry through a single city. The large, uncentered slave trade and the large slaveholding units assured the survival not only of the common denominators of West African culture but also many of its particular tribal and national forms. Planter preferences or perhaps the chance ascendancy of one group sometimes allowed specific African cultures to reconstitute themselves within the plantation setting. To be sure, Africans changed in the lowcountry. Even where blacks enjoyed numerical superiority and a considerable degree of auton-

⁴⁴ By the middle of the nineteenth century, the work required under the task system had been carefully defined. Indeed, for many lowcountry crops, the task had become so standardized that it was often used interchangeably as a unit of land (the amount necessary to grow a task of peas) or even a unit of time (the amount of time it took to plant a task of peas). Nevertheless, the struggle over the definition of the task did not end. Following emancipation, when planters attempted to eliminate the task system, freed people objected, often violently. In 1865, a Union soldier reported from Georgetown that the freedmen "have been accustomed to working by task, which has always given them leisure to cultivate land for themselves, tend their stock, and amuse themselves, and, therefore very correctly, I think, [believe] that with such a change in the march of labor all their privileges will go and their condition will be less to their taste than it was when they were slaves." Lt. Col. A. J. Willard to Capt. George H. Hooker, Georgetown, November 7, 1865, Letters Sent, vol. 156 DS, U.S. Army Commands, RG 393, pt. 2, National Archives, Washington, D.C. Also see Ulrich B. Phillips, ed., *Plantation and Frontier*, 1 (Cleveland, 1909): 115-19; and Frederick Law Olmsted, *The Cotton Kingdom*, ed. Arthur M. Schlesinger, 1 (New York, 1953): 190-94. The origins of the task system and the struggle over the definition of work in the eighteenth century has not yet been investigated, but, for the kinds of disputes that defined the measure of a task, see Josiah Smith to George Austin, July 22, 1773, Josiah Smith Letterbook, Southern Historical Collection, and Richard Hutson to Mr. Croll, "per Caser," August 22, 1767, Charles W. Hutson Papers, Southern Historical Collection, University of North Carolina, Chapel Hill.

omy, they could no more transport their culture unchanged than could their masters. But lowcountry blacks incorporated more of West African culture—as reflected in their language, religion, work patterns, and much else—into their new lives than did other black Americans. Throughout the eighteenth century and into the nineteenth, lowcountry blacks continued to work the land, name their children, and communicate through word and song in a manner that openly combined African traditions with the circumstances of plantation life.⁴⁵

The new pattern of creolization that developed following the rice revolution smashed the emerging homogeneity of black life in the first years of settlement and left lowcountry blacks deeply divided. One branch of black culture evolved in close proximity to whites. Urban, often skilled, well-traveled, and increasingly American-born, creoles knew white society well, and they used their knowledge to better themselves. Some, clearly a well-connected minority, pressed for incorporation into the white world. They urged missionary groups to admit their children to school and later petitioned lawmakers to allow their testimony in court, carefully adding that they did not expect full equality with whites.⁴⁶ Plantation slaves shared few of the assimilationist aspirations of urban creoles. By their dress, language, and work routine, they lived in a world apart. Rather than demand incorporation into white society, they yearned only to be left alone. Within the quarter, aided by their numerical dominance, their plantation-based social hierarchy, and their continued contact with Africa, they developed their own distinctive culture, different not only from that of whites but also from the cosmopolitan world of their Afro-American brethren. To be sure, there were connections between the black majority and the urban creoles. Many—market women, jobbing artisans, and boatmen—moved easily between these two worlds, and most blacks undoubtedly learned something of the other world through chance encounters, occasional visits, and word of mouth.⁴⁷ Common white oppression continually shrank the social distance that the distinctive experience created, but by the eve of the Revolution, deep cultural differences separated those blacks who sought to improve their lives through incorporation into the white world and those who determined to disregard the white man's ways. If the movement from African to creole obliterated cultural differences among Northern blacks, creolization fractured black society in the lowcountry.

CULTURAL DISTINCTIONS BETWEEN AFRICANS AND AFRO-AMERICANS developed in the Chesapeake as well, although the dimension of differences between African and creole tended to be time rather than space. Unlike in the lowcountry, white

⁴⁵ Wood, *Black Majority: Negroes in Colonial South Carolina*, esp. chap. 6; Lorenzo D. Turner, *Africanisms in the Gullah Dialect* (Chicago, 1949); William R. Bascom, "Acculturation among the Gullah Negroes," *American Anthropologist*, 43 (1941): 43–50; Klingberg, *An Appraisal of the Negro in South Carolina*; and Hennig Cohen, "Slave Names in Colonial South Carolina," *American Speech*, 28 (1952): 102–07.

⁴⁶ Klingberg, *An Appraisal of the Negro in Colonial South Carolina*, 116–17; and Petition of John and William Morriss, 1791, and Petition from Camden Negroes, 1793, South Carolina Legislative Papers, South Carolina Department of Archives and History, Columbia.

⁴⁷ For one planter's attempt to keep boatmen from mixing with his plantation hands, see Laurens, *Papers of Henry Laurens*, 4: 319, 633; and Sellers, *Charleston Business on the Eve of the American Revolution*, 108.

planters did not promote the creation of a distinctive group whose origins, function, and physical appearance distinguished them from the mass of plantation slaves and offered them hope, however faint, of eventual incorporation into white society. And, compared to the North, African immigration into the Chesapeake came relatively early in the process of cultural transformation. As a result, African-creole differences disappeared with time and a single, unified Afro-American culture slowly emerged in the Chesapeake.

As in the lowcountry, little distinguished black and white laborers during the early years of settlement. Most of the first blacks brought into the Chesapeake region were West Indian creoles who bore English or Spanish surnames and carried records of baptism. Along the James, as along the Cooper, the demands of pioneer life at times operated to strengthen the slaves' bargaining position. Some blacks set the condition of their labor, secured their family life, participated in the region's internal economy, and occasionally bartered for their liberty. This, of course, did not save most black people from the brutal exploitation that almost all propertyless men and women faced as planters squeezed the last pound of profit from the tobacco economy. The blacks' treatment at the hands of planters differed little from that of white bound labor in large measure because it was difficult to treat people more brutally.⁴⁸ While the advantages of this peculiar brand of equality may have been lost on its beneficiaries, those blacks who were able to complete their terms of servitude quickly joined whites in the mad scramble for land, servants, and status.

Many did well. During the seventeenth century, black freemen could be found throughout the region owning land, holding servants, and occasionally attaining minor offices. Like whites, they accumulated property, sued their neighbors, and passed their estates to their children. In 1651, Anthony Johnson, the best known of these early Negro freemen, received a two-hundred-and-fifty-acre headright for importing five persons into Virginia. John Johnson, a neighbor and probably a relative, did even better, earning five hundred and fifty acres for bringing eleven persons into the colony. Both men owned substantial farms on the Eastern Shore, held servants, and left their heirs sizable estates. As established members of their communities, they enjoyed the rights of citizens. When a servant claiming his freedom fled Anthony Johnson's plantation and took refuge with a nearby white farmer, Johnson took his neighbor to court and won the return of his servant along with damages against the white man.⁴⁹

The class rather than racial basis of early Chesapeake society enabled many black men to compete successfully for that scarcest of all New World commodities: the affection of white women. Bastardy lists indicate that white female servants ignored the strictures against what white lawmakers labeled "shame-

⁴⁸ Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 108–79, 215–49; and Wesley Frank Craven, *White, Red, and Black: The Seventeenth-Century Virginian* (Charlottesville, Va., 1971), 75–99.

⁴⁹ Ross M. Kimmel, "Free Blacks in Seventeenth-Century Maryland," *Maryland Historical Magazine*, 71 (1976): 19–25; John H. Russell, *The Free Negro in Virginia, 1619–1865* (Baltimore, 1913), 24–38, 88, 116, 119–20, 136–37; James H. Brewer, "Negro Property Owners in Seventeenth-Century Virginia," *William and Mary Quarterly*, 3d ser., 12 (1955): 575–80; and Susie M. Ames, *Studies of the Virginia Eastern Shore in the Seventeenth Century* (Richmond, Va., 1940), 99–108.

ful" and "unnatural" acts and joined together with men of their own condition regardless of color. Fragmentary evidence from various parts of seventeenth-century Virginia reveals that approximately one-quarter to one-third of the bastard children born to white women were mulattoes. The commonplace nature of these interracial unions might have been the reason why one justice legally sanctified the marriage of Hester, an English servant woman, to James Tate, a black slave. Some successful, property-owning whites and blacks also intermarried. In Virginia's Northampton county, Francis Payne, a Negro freeman, married a white woman, who later remarried a white man after Payne's death. William Greensted, a white attorney who represented Elizabeth Key, a mulatto woman, in her successful suit for her freedom, later married her. In 1691, when the Virginia General Assembly finally ruled against the practice, some propertied whites found the legislation novel and obnoxious enough to muster a protest.⁵⁰

By the middle of the seventeenth century, Negro freemen sharing and fulfilling the same ideals and aspirations that whites held were no anomaly in the Chesapeake region. An Eastern Shore tax list of 1668 counted nearly a third of black tithables free. If most blacks did not escape the tightening noose of enslavement, they continued to live and work under conditions not much different from white servants. Throughout the seventeenth and into the first decades of the eighteenth century, black and white servants ran away together, slept together, and, upon occasion, stood shoulder to shoulder against the weighty champions of established authority. Thus viewed from the first years of settlement—the relatively small number of blacks, their creole origins, and the initial success of some in establishing a place in society—black acculturation in the Chesapeake appeared to be following the nonplantation pattern of the Northern colonies and the pioneer lowcountry.⁵¹

THE emergence of a planter class and its consolidation of power during a series of political crises in the middle years of the seventeenth century transformed black life in the Chesapeake and threatened this pattern of cultural change. Following the legalization of slavery in the 1660s, black slaves slowly but steadily replaced white indentured servants as the main source of plantation labor. By 1700, blacks made up more than half the agricultural work force in Virginia and, since the great planters could best afford to purchase slaves, blacks composed an even larger share of the workers on the largest estates. Increased reliance on slave labor quickly outstripped West Indian supplies. Beginning in the

⁵⁰ Morgan, *American Slavery, American Freedom*, 329–37; Warren M. Billings, "The Cases of Fernando and Elizabeth Key: A Note on the Status of Blacks in the Seventeenth Century," *William and Mary Quarterly*, 3d ser., 30 (1973): 467–74; and Kimmel, "Free Blacks in Seventeenth-Century Maryland," 20–21.

⁵¹ Edmund S. Morgan, "Slavery and Freedom: The American Paradox," *Journal of American History*, 59 (1972): 17–18; and T. H. Breen, "A Changing Labor Force and Race Relations in Virginia, 1660–1710," *Journal of Social History*, 7 (1973): 3–25. The confused, uncertain status of black people generally and of free blacks in particular during the seventeenth century also indicates the unwillingness, inability, or, more probably, lack of interest on the part of whites in firmly fixing the status of blacks. For the farrago of legislation governing free blacks, see Ira Berlin, *Slaves without Masters: The Free Negro in the Antebellum South* (New York, 1974), 7–9; and Jordan, *White over Black*, 136–78. The status of blacks, free or slave, has become something of a historical perennial, with scholars agreeing that before the 1660s at least some blacks were free and some were slave and the precise status of most is simply impossible to determine. For a review of the evidence, see Jordan, *White over Black*, chap. 2.



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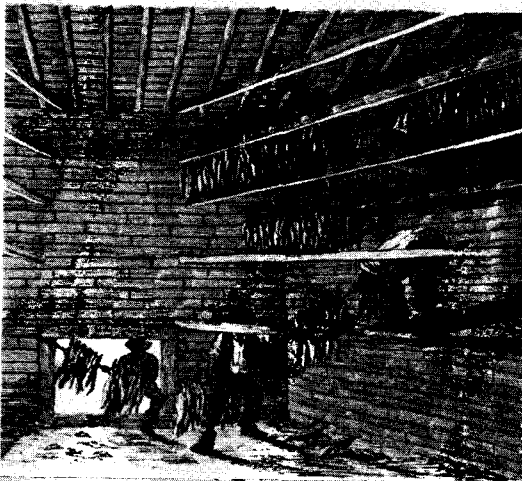
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Figure 2: Late nineteenth-century representation of the stages of prerevolutionary tobacco production. 1: Sowing and hilling. 2: Transplanting. 3: Laying by, topping, worming, and suckering. 4: Cutting and sticking. 5: Housing. 6: Stripping and tying. Photograph taken from Benjamin Butterworth, *The Growth of Industrial Art* (Washington, 1892), 192.

1680s, Africans entered the region in increasingly large numbers. The proportion of blacks born in Africa grew steadily throughout the waning years of the seventeenth century, so that by the first decade of the eighteenth century, Africans composed some three-quarters of the region's blacks.⁵² Unlike the low-country, African imports never threatened the Chesapeake's overall white numerical superiority, but by the beginning of the eighteenth century they dominated black society. Some eighty years after the first blacks arrived at Jamestown and some forty years after the legalization of slavery, African importation profoundly transformed black life.

Slave conditions deteriorated as their numbers increased. With an eye for a quick profit, planters in the Chesapeake imported males disproportionately. Generally men outnumbered women more than two to one on Chesapeake slavers. Wildly imbalanced sex ratios undermined black family life. Physically spent and emotionally drained by the rigors of the Middle Passage, African women had few children. Thus, as in the North and the Carolina lowlands, the black birth rate fell and mortality rate surged upward with the commencement of direct African importation.⁵³

The hard facts of life and death in the Chesapeake region distinguished creoles and Africans at the beginning of the eighteenth century. The demands of the tobacco economy enlarged these differences in several ways. Generally, planters placed little trust in newly arrived Africans with their strange tongues and alien customs. While they assigned creoles to artisanal duties on their plantations and to service within their households, they sent Africans to the distant, upland quarters where the slaves did the dull, backbreaking work of clearing the land and tending tobacco. The small size of these specialized upcountry units, their isolation from the mainstream of Chesapeake life, and their rude frontier conditions made these largely male compounds lonely, unhealthy places that narrowed men's vision. The dynamics of creole life, however, broadened black understanding of life in the New World. Traveling freely through the countryside as artisans, watermen, and domestic servants, creoles gained in confidence as they mastered the terrain, perfected their English, and learned about Christianity and other cultural modes that whites equated with civilization. Knowledge of the white world enabled black creoles to manipulate their masters to their own advantage. If Afro-Americans became increasingly knowledgeable about their circumstances and confident of their ability to deal with them, Africans remained provincials, limited by the narrow alternatives of plantation life.⁵⁴

⁵² Allan Kulikoff, "A 'Prolifick' People: Black Population Growth in the Chesapeake Colonies, 1700-1790," *Southern Studies*, 16 (1977): 391-96, 403-05, and "The Origins of Afro-American Society in Tidewater Maryland and Virginia, 1700 to 1790," *William and Mary Quarterly*, 3d ser., 35 (1978): 229-31; Russell R. Menard, "The Maryland Slave Population, 1658 to 1730: A Demographic Profile of Blacks in Four Counties," *ibid.*, 32 (1975): 30-32; and Craven, *White, Red, and Black*, 89-103. Herbert S. Klein has maintained that West Indian re-exports remained the majority into the first two decades of the eighteenth century; see his "Slaves and Shipping in Eighteenth-Century Virginia," *Journal of Interdisciplinary History*, 5 (1975): 384-85.

⁵³ Kulikoff, "A 'Prolifick' People: Black Population Growth," 392-406; Menard, "The Maryland Slave Population," 30-35, 38-49; and Craven, *White, Red, and Black*, 98-101.

⁵⁴ Gerald W. Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia* (New York, 1972), esp. chaps. 2-3; Menard, "The Maryland Slave Population," 32-54; and Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 236-49.

As in the lowcountry and the Northern colonies, Africans in the Chesapeake strove to escape whites, while creoles used their knowledge of white society for their own benefit. These cultural differences, which were reflected in all aspects of black life, can be seen most clearly in the diverse patterns of resistance. Africans ran away toward the back country and isolated swamps. They generally moved in groups that included women and children, despite the hazards such groups entailed for a successful escape. Their purpose was to recreate the only society they knew free from white domination. In 1727, Governor William Gooch of Virginia reported that about a dozen slaves had left a new plantation near the falls of the James River. They headed west and settled near Lexington, built houses, and planted a crop before being retaken. But Afro-Americans ran away alone, usually with the hope of escaping into American society. Moving toward the areas of heaviest settlement, they found refuge in the thick network of black kinship that covered the countryside and sold their labor to white yeomen with few questions asked. While the possibility of passing as free remained small in the years before the Revolution, the creoles' obvious confidence in their ability to integrate themselves into American society stands in stark contrast to that of Africans, who sought first to flee it.⁵⁵

As reflected in the mode of resistance, place of residence, occupation, and much else, Africans and creoles developed distinctive patterns of behavior and belief. To a degree, whites recognized these differences. They stigmatized Africans as "outlandish" and noted how creoles "affect our language, habits, and customs." They played on African-creole differences to divide blacks from each other, and they utilized creole skills to maximize the benefits of slave labor. But this recognition did not elevate creoles over Africans in any lasting way. Over the course of the century following legal enslavement, it had precisely the opposite effect. Chesapeake planters consolidated their class position by asserting white racial unity. In this context, the entry of large numbers of African—as opposed to creole—blacks into the region enlarged racial differences and helped secure planter domination. Thus, as reliance on black labor increased, the opportunities for any black—no matter how fluent in English or conversant with the countryside—to escape bondage and join the scramble for land, servants, and status diminished steadily.

By the middle of the eighteenth century, the size and character of the free Negro population had been significantly altered. Instead of a large minority of the black population, Negro freemen now composed just a small proportion of all blacks, probably not more than 5 percent. Many were cripples and old folks whom planters discarded when they could no longer wring a profit from their labor. While most were of mixed racial origins, few of these free mulattoes of the Chesapeake, in contrast to those of the lowcountry, traced their ancestry to the planter class. Instead, they descended from white servants, frequently women. These impoverished people had little status to offer their children. Indeed, planter-inspired legislation further compromised their liberty by requiring that

⁵⁵ Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 34–110, esp. table 3 (pp. 108–09); and Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 253–54.

the offspring of white women and black men serve their mother's master for thirty-one years. Those who survived the term could scarcely hope for the opportunities an earlier generation of Negro freemen had enjoyed.⁵⁶ The transformation of the free Negro caste in the century between 1660 and 1760 measured the change in Chesapeake society as its organizing principle changed from class to race.

The free Negro's decline reveals how the racial imperatives of Chesapeake society operated to lump all black people together, free and slave, creole and African. In the Chesapeake, planters dared not grant creoles special status at the expense of Africans. Since the Africans would shortly be creoles and since creoles shared so much with whites, distinctions among blacks threatened the racial division that underlay planter domination. In the lowcountry, where geography, economy, and language separated white and black, those few blacks who spoke, dressed, acted, and looked like whites might be allowed some white prerogatives. But, if lowcountry planters could argue that no white man could do the work required to grow rice commercially, no one in the Chesapeake could reasonably deny that whites could grow tobacco. The fundamental unity of Chesapeake life and the long-term instability of African-creole differences pushed blacks together in the white mind and in fact.

During the middle years of the eighteenth century, changes in the Chesapeake economy and society further diminished differences within black society and created a unified Afro-American culture. The success of the tobacco economy enlarged the area of settlement and allowed planters to increase their holdings. The most successful planters, anxious to protect themselves from the rigors of the world marketplace, strove for plantation self-sufficiency. The great estates of the Chesapeake became self-contained enterprises with slaves taking positions as artisans, tradesmen, wagoners, and, sometimes, managers; the plantation was "like a Town," as a tutor on Robert Carter's estate observed, "but most of the Inhabitants are black." The increased sophistication of the Chesapeake economy propelled many more blacks into artisanal positions and the larger units of production, tighter pattern of settlement, and the greater mobility allowed by the growing network of roads ended the deadening isolation of the upcountry quarter. Bondsmen increasingly lived in large groups, and those who did not could generally find black companionship within a few miles' walk. Finally, better food, clothing, and shelter and, perhaps, the development of immunities to New World diseases enabled blacks to live longer, healthier lives.⁵⁷

⁵⁶ Hugh Jones, *The Present State of Virginia* (1724), ed. Richard L. Morton (Chapel Hill, N.C., 1956), 75; Berlin, *Slaves without Masters*, 3-6; Donald L. Horowitz, "Color Differentiation in the American Systems of Slavery," *Journal of Interdisciplinary History*, 3 (1973): 526-30; and George M. Fredrickson, "Toward a Social Interpretation of the Development of American Racism," in Nathan I. Huggins *et al.*, eds., *Key Issues in the Afro-American Experience*, 1 (New York, 1971): 246-47.

⁵⁷ Philip V. Fithian, *The Journal and Letters of Philip Vickers Fithian, 1773-1774*, ed. Hunter D. Farish (Williamsburg, Va., 1943), 73; Mullin, *Flight and Resistance: Slave Resistance in Eighteenth-Century Virginia*, 19-32; Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 240-42, 246-49; Louis Morton, *Robert Carter of Nomini Hall: A Virginia Tobacco Planter of the Eighteenth Century* (Charlottesville, Va., 1941); Michael Greenberg, "William Byrd II and the World of the Market," *Southern Studies*, 16 (1977): 429-56; and, especially, Landon Carter, *The Diary of Colonel Landon Carter of Sabine Hall, 1752-1778*, ed. Jack P. Greene, 2 vols. (Charlottesville, Va., 1966), *passim*.

As part of their drive for self-sufficiency, Chesapeake slaveholders encouraged the development of an indigenous slave population. Spurred by the proven ability of Africans to survive and reproduce and pressed in the international slave market by the superior resources of West Indian sugar magnates and lowland rice growers, Chesapeake planters strove to correct the sexual imbalance within the black population, perhaps by importing a large proportion of women or lessening the burden of female slaves. Blacks quickly took advantage of this new circumstance and placed their family life on a firmer footing. Husbands and wives petitioned their owners to allow them to reside together on the same quarter and saw to it that their families were fed, beyond their masters' rations. Planters, for their part, were usually receptive to slaves' demands for a secure family life, both because it reflected their own values and because they profited mightily from the addition of slave children. Thomas Jefferson frankly considered "a woman who brings a child every two years as more profitable than the best man on the farm [for] what she produces is an addition to capital, while his labor disappears in mere consumption." Under these circumstances, the black population increased rapidly. Planters relied less and less on African importation and, by the 1740s, most of the growth of the black population came from natural increase. Within a generation, African importation was, for all practical purposes, no longer a significant source of slave labor. In the early 1770s, the period of the greatest importation into the lowcountry, only five hundred of the five thousand slaves added annually to the black population of Virginia derived directly from Africa.⁵⁸

The establishment of the black family marked the re-emergence of Afro-American culture in the Chesapeake. Although Africans continued to enter the region, albeit at a slower pace, the nature of the slave trade minimized their impact on the development of black society in the region. Unlike those in the lowcountry, newly arrived Africans could rarely hope to remain together. Rather than funnel their cargo through a single port, Chesapeake slavers peddled it in small lots at the many tobacco landings that lined the bay's extensive perimeter. Planters rarely bought more than a few slaves at a time, and larger purchasers, usually the great planter-merchants, often acted as jobbers, quickly reselling these slaves to backcountry freeholders.⁵⁹ The resulting fragmentation sent

⁵⁸ Allan Kulikoff, "The Beginnings of the Afro-American Family in Maryland," in Aubrey C. Land *et al.*, eds., *Law, Society, and Politics in Early Maryland* (Baltimore, 1977), 177–96, "A 'Prolifick' People: Black Population Growth," 401–03, 405–14, and "Origins of Afro-American Society in Tidewater Maryland and Virginia," 246–53; Daniel Dulany to Robert Carter, December 18, 1768, Colonial Papers, Maryland Historical Society, Baltimore; Robert Carter to John Pound, March 16, 1779, to Fleet Cox, January 2, 1788, and to George Newman, December 29, 1789, typescript, Robert Carter Papers, Duke University, Durham, N.C.; John C. Fitzpatrick, ed., *The Writings of George Washington*, 39 vols. (Washington, 1931–44), 2: 526, 29: 154, 398; and Edwin M. Betts, ed., *Thomas Jefferson's Farm Book* (New York, 1953), pt. 2: 46, 12–13, 21, 24–26, 42–46. Planters also found a relationship between family stability and social stability. A Maryland planter instructed his overseer about a returned fugitive: "While his wife continues at home, I suppose there will be no danger of his making a second attempt to get off. You may let him know, that his pardon depends upon his good future behavior, that if he behaves well, and endeavours to make amends for his past behavior I will when I return purchase his wife if her master will sell her at a reasonable price." Letter of John Hanson, January 29, 1782, John Hanson Papers, Maryland Historical Society, Baltimore.

⁵⁹ Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 14–16; Kulikoff, "Origins of Afro-American Society in Tidewater Maryland and Virginia," 230–35; Darold D. Wax, "Black Immigrants: The

newly arrived Africans in all directions and prevented the maintenance of tribal or shipboard ties. Chesapeake slaveholders cared little about the origins of their slaves. In their eyes, newly arrived Africans were not Iboes, Coromantees, or Angolans, but "new Negroes." While the unicentered slave trade sustained and strengthened African culture in the lowcountry, the Chesapeake slave trade facilitated the absorption of Africans into the evolving creole society.

Differences between creoles and Africans did not disappear with the creation of a self-sustaining Afro-American population. The creoles' advantages—language skills, familiarity with the countryside, artisanal standing, and knowledge of the plantation routine—continued to propel them into positions of authority within the slave hierarchy. In some ways, the growing complexity of the Chesapeake economy widened the distance between Africans and creoles, at least at first. Most of the skilled and managerial positions within the region's expanding iron industry went to creole blacks as did the artisanal work in flour mills and weaving houses. On some plantations, moreover, artisan and house status became lodged in particular families with parents passing privileged positions on to their children. Increasingly, skilled slaves entered the market economy by selling their own time and earning money from "overwork," thereby gaining a large measure of freedom. For the most part, Africans remained on rude, backwoods plantations tending the broad-leaf weed. Since creole slaves sold at a premium price and most great planters had already established self-sustaining slave forces, small planters purchased nearly all of the newly arrived Africans after mid-century. These upward-striving men generally owned the least developed, most distant farms. Their labor requirements remained primitive compared to the sophisticated division of labor on the self-contained plantation-towns.⁶⁰

Over the long term, however, economic changes sped the integration of Africans into Afro-American society. Under the pressure of a world-wide food shortage, Chesapeake planters turned from the production of tobacco to that of foodstuff, especially wheat. The demands of wheat cultivation transformed the nature of labor in the region. Whereas tobacco farming required season-long labor, wheat farming employed workers steadily only during planting and harvesting. The remainder of the year, laborers had little to do with the crop. At the same time, however, wheat required a larger and more skilled labor force to transport the grain to market and to store it, mill it, and reship it as flour, bread, or bulk grain. Economic changes encouraged masters to teach their slaves skills and to hire them out during the slack season. At first, these opportunities went mostly to creoles, but as the wheat economy grew, spurring urbanization and manufacturing, the demands for artisans and hirelings outstripped the creole

Slave Trade in Colonial Maryland," *Maryland Magazine of History*, 73 (1978): 30-45; and Winthrop D. Jordan, "Planter and Slave Identity Formation: Some Problems in the Comparative Approach," in Rubin and Tuden, *Comparative Perspectives on Slavery in New World Plantation Societies*, 38.

⁶⁰ Kulikoff, "The Beginnings of the Afro-American Family in Maryland," 185-86; Jordan, *White over Black*, 405 n. 7; Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 83-139; "Description of Servants, 1772," Northampton Furnace, Ridgely Account Books, Maryland Historical Society, Baltimore; and Ronald L. Lewis, *Coal, Iron, and Slaves: Industrial Slavery in Maryland and Virginia, 1715-1865* (Westport, Conn., 1979), 82-84, 162-63.

population.⁶¹ An increasing number of Africans were placed in positions previously reserved for creoles. The process of cultural transformation that earlier in the eighteenth century had taken a generation or more was considerably shorter at mid-century. Africans became Afro-Americans with increasing rapidity as the century wore on, eliminating the differences within black society that African importation had created.

Chesapeake blacks enjoyed considerably less autonomy than their lowcountry counterparts. Resident planters, small units of production, and the presence of large numbers of whites meant that most blacks lived and worked in close proximity to whites. While lowcountry planters fled to coastal cities for a large part of the year, the resident planter was a fixture of Chesapeake life. Small freeholders labored alongside slaves, and great planters prided themselves on regulating all aspects of their far-flung estates through a combination of direct personal supervision and plantation-based overseers. The latter were usually white, drawn from the region's white majority. Those few blacks who achieved managerial positions, moreover, enjoyed considerably less authority than lowland drivers. The presence of numerous nonslaveholding whites circumscribed black opportunities in other ways as well. While Chesapeake slaves commonly kept gardens and flocks of barnyard animals, white competitors limited their market and created a variety of social tensions. If lowcountry masters sometimes encouraged their slaves to produce nonstaple garden crops, whites in the Chesapeake—slaveholders and nonslaveholders alike—complained that blacks stole more than they raised and worked to curb the practice. Thus, at every turn, economy and society conspired to constrain black autonomy.

The requirements of tobacco cultivation reinforced the planters' concern about daily work routine. Whereas the task system insulated lowcountry blacks against white intervention and maximized black control over their work, the constant attention demanded by tobacco impelled Chesapeake planters to oversee the tedious process of cultivating, topping, worming, suckering, and curing tobacco. The desire of Chesapeake masters to control their slaves went beyond the supervision of labor. Believing that slaves depended on them "for every necessity of life," they intervened in the most intimate aspects of black life. "I hope you will take care that the Negroes both men and women I sent you up last always go by the names we gave them," Robert "King" Carter reminded his steward. "I am sure we repeated them so often . . . that everyone knew their names & would readily answer to them." Chesapeake planters sought to shape domestic

⁶¹ Carville Earle and Ronald Hoffman, "The Urban South: The First Two Centuries," *Perspectives in American History*, 10 (1976): 26–76; Mullin, *Flight and Rebellion: Slave Resistance in Eighteenth-Century Virginia*, 87–88, 124–27; and Gray, *Agriculture in the Southern United States*, 2: 602–17. Although the best study of slave hiring in the Chesapeake region focuses on the post-Revolution years, the forces promoting slave hire after the war suggest that the practice predates the Revolution. See Sarah S. Hughes, "Slaves for Hire: The Allocation of Black Labor in Elizabeth City County, Virginia, 1782 to 1810," *William and Mary Quarterly*, 3d ser., 35 (1978): 260–86. Also see Robert Carter to Warner Lewis, October 16, 1773, and October 20, 1774, to Mrs. Corbin, September 27, 1775, to Griffin Garland, September 29, 1775, and to John Ballantine, July 7, 1777, Carter Papers, typescript, Duke University, Durham, N.C. Allan Kulikoff has estimated that the proportion of blacks working as agricultural laborers dropped from 90 to 82 percent between 1733 and 1776; see his "Tobacco and Slaves: Population, Economy, and Society in Eighteenth-Century Prince George's County, Maryland" (Ph.D. dissertation, Brandeis University, 1976), 235–39.

relations, cure physical maladies, and form personalities. However miserably they failed to ensure black domestic tranquility and reform slave drunkards, paternalism at close quarters in the Chesapeake had a far more potent influence on black life than the distant paternalism that developed in the lowcountry. Chesapeake blacks developed no distinct language and rarely utilized African day names for their children.⁶² Afro-American culture in the Chesapeake evolved parallel with Anglo-American culture and with a considerable measure of congruence.

THE DIVERSE DEVELOPMENT OF AFRO-AMERICAN CULTURE during the seventeenth and eighteenth centuries reveals the importance of time and place in the study of American slavery. Black people in colonial America shared many things: a common African lineage, a common racial oppressor, a common desire to create the richest life possible for themselves and their posterity in the most difficult of circumstances. But these commonalities took different shape and meaning within the diverse circumstances of the North American mainland. The nature of the slave trade, the various demographic configurations of whites and blacks, and the demands of particular staples—to name some of the factors influencing the development of slave society—created at least three distinctive patterns of Afro-American life. Perhaps a finer analysis will reveal still others.

This diversity did not end with the American Revolution. While African-creole differences slowly disappeared as the centerpole of black society with the closing of the slave trade and the steady growth of an Afro-American population, other sources of cohesion and division came to the fore.⁶³ Differences between freemen and bondsmen, urban and rural folk, skilled and unskilled workers, and browns and blacks united and divided black people, and made black society every bit as variable and diverse during the nineteenth century as in the eighteenth. Indeed the diversity of black life increased substantially during the antebellum years as political changes abolished slavery in some places and strengthened it in others, as demographic changes set in motion by the Great Migration across the Lower South took effect, as the introduction of new crops enlarged the South's repertoire of staples, and as the kaleidoscopic movement of the world market sent the American economy in all directions.

⁶² Robert Carter to Robert Jones, Robert "King" Carter Letterbooks, Alderman Library, University of Virginia, Charlottesville (I am grateful to Emory Evans for alerting me to this letter); and Robert Carter to William Carr, March 15, 1785, Carter Papers, typescript, Duke University, Durham, N.C. Also see Carter, *Diary of Colonel Landon Carter of Sabine Hall*, *passim*; Robert Carter to his various stewards and overseers (Rubin Sanford, Clement Brooke, Newyear Branson), Carter Papers, typescript, Duke University, Durham, N.C.; Fitzpatrick, *The Writings of George Washington*, esp. vols. 32–34; Depositions of James Holland, William Ferguson, and Charles Gardiner, August 23, 1793, Lloyd Family Papers, Maryland Historical Society, Baltimore; and Betts, *Thomas Jefferson's Farm Book*, pt. 2: 16. For the striking difference in naming patterns of Chesapeake and low-country bondsmen, compare the slave lists in the Charles Carroll Account Book, Maryland Hall of Records, Annapolis, and the Charles C. Pinckney Plantation Journal, Manuscript Division, Library of Congress, Washington, D.C.

⁶³ For the importance of African-creole differences in understanding black reactions to the revolutionary crises of the last quarter of the eighteenth century, see Michael Mullin, "British Caribbean and North American Slaves in an Era of War and Revolution, 1775–1807," in Jeffrey J. Crow and Larry E. Tise, eds., *The Southern Experience in the American Revolution* (Chapel Hill, N.C., 1978), 235–67.

If slave society during the colonial era can be comprehended only through a careful delineation of temporal and spatial differences among Northern, Chesapeake, and lowcountry colonies, a similar division will be necessary for a full understanding of black life in nineteenth-century America. The actions of black people during the American Revolution, the Civil War, and the long years of bondage between these two cataclysmic events cannot be understood merely as a function of the dynamics of slavery or the possibilities of liberty, but must be viewed within the specific social circumstances and cultural traditions of black people. These varied from time to time and from place to place. Thus no matter how complete recent studies of black life appear, they are limited to the extent that they provide a static and singular vision of a dynamic and complex society.

Reviews of Books

GENERAL

QUENTIN SKINNER. *The Foundations of Modern Political Thought*. Volume 1, *The Renaissance*; volume 2, *The Age of Reformation*. New York: Cambridge University Press. 1978. Pp. xxiv, 305; vi, 405. Cloth \$29.50, paper \$9.50 each.

Quentin Skinner professes three aims in writing his monumental study of the "foundations of modern political thought": he wishes to present a simple outline of the principal texts; he wants to indicate "the process by which the modern concept of the State came to be formed"; and he attempts to exemplify his ideas of how historical texts should be studied, namely, by placing "the leading theorists" in "the more general social and intellectual matrix out of which their works arose." It is fair to say that, with some cavils, he has accomplished these goals. As a matter of observed fact he seems to me to have presented a remarkably detailed summary of almost all major and minor thinkers who contributed something of significance to either their own time's conceptions of politics or ours; moreover, with the exception of a very few of the latest works, he has brought to bear just about every worthwhile interpretive study of the last half-century that may be thought to throw light on his subject (essentially by English-speaking scholars, but including some in French, German, and Italian). If it were not for the fact that, though extremely broadly defined, his subject is yet limited to the history of political thought, one would be tempted to recommend these volumes as an up-to-date interpretive survey of the period as a whole in the best English university style of historical synthesis. His judgments are (from my own point of view, of course) nearly impeccable in the more controversial corners of this broad field. Common sense, wide reading, and methodological sophistication are smoothly and pleasingly blended in an altogether satisfying way for both the author and a doubtlessly wide range of readers.

And yet, despite so many very admirable features of this large and perfectionistic work, and even in

the core of probably its strongest achievement, the worm of discontent still squirms. I like his contextual approach very much. The book is organized into a succession of clusters of schools, sects, and movements, each of which makes an important contribution to the development of political thought and constitutes a subculture of the period as a whole. Within each, a collection of minor or major figures presents a collective ideology that provides the parameters within which rulers and other political agents may act in Skinner's view, although this is not always explicitly demonstrated. Thus it is his contention, and dearly held, that political thought, viewed in this collectivistic, ideological way, attains a general historical importance lacking in isolated, history-of-ideas treatments of famous men or great thinkers. He manages in this method to be both an internalist, emphasizing the particular tradition and interests from which a given thinker emerges, and an externalist, viewing the subculture as having a broad impact on the larger historical situation. Here one might observe a less explicit but always implied concern with the impact of events and structures, in their turn, on ideas and ideologies.

The discontent one feels may well be inherent in the author's compulsion to present so many not very interesting lesser figures who indeed provide the context out of which a Machiavelli, or a Thomas More, or a Jean Bodin arises. My approbation for dealing with intellectual history culturally and collectively in this fashion is necessarily strong, because I have practiced it and constantly advocate it myself. But one must confess to missing the sense of the dramatic that a sharper focusing on and featuring of the truly revolutionary minds would provide. Such thinkers took the givens of their group and even polemicized and ridiculed them in order to bring forth what in the end mattered more in public life and to reveal, perhaps, the true contribution of the boring, routinized thinking of the schools that fathered the more daring ones. It is not that Machiavelli and More were not "true" humanists of their own time and place but that they made the issues that underlay the growling and quarreling of

their day bright and clear to be faced as challenges or, more likely, paid lip service to and embarrassingly ignored.

Skinner deals with Machiavelli's and More's critiques of other humanists in the last chapter of volume one, and he follows Felix Gilbert and J. H. Hexter in his interpretation. Though he intends the reader to see a continuity between the *ragione di stato* he sees in Machiavelli (following Meinecke/Gilbert) and the clarity of Buchanan's secularist affirmation of the objectivity of sovereignty, the issue is somehow obscured by the complexity of his exposition of German, English, French, Italian, and Spanish Reformation and Counter-Reformation controversies. If such continuity, in history, really is the issue!

And yet the demonstrated continuity from the late *duecento* city-state struggle for *libertas*—independence and self-government—to the late sixteenth-century contest between proponents of monarchical and popular sovereignty, as Skinner has presented it, is indeed massive. And his effort to relate it to theological and ecclesiastical issues is never failing. But for this reviewer, and without the least sense of attempting to promote a populist perspective, this concentration—the concentration of the political philosopher—on the status of the state seems genetic-modernizing and retrospective, doing full justice neither to the minds and passions of the major thinkers nor to the religious, moral, familial, and survivalistic concerns of the great mass of the population. Was the condition and destiny of mankind totally determined by divine providence, or did man himself have a role? What kind, how much, under what circumstances? The center of consciousness of those centuries of Europe's history lay in such questions, not in revival of ancient and in the foundation of modern political thought.

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DIETMAR ROTHERMUND *Europa und Asien im Zeitalter des Merkantilismus*. (Erträge der Forschung, number 80.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1978. Pp. vi, 184.

Prompted by the paradox that during the "Age of Mercantilism (1602–1775)" Europe's trade with Asia displayed an antimercantilist character, Dietmar Rothermund has produced an exceedingly useful overview of that trade. He has drawn together the results of research in several areas and has suggested a provocative synthesis.

In the first place, he stresses the pivotal importance of the Asian trade for European economic development. It was, as Rothermund puts it, an *angel-punkt*. The herring trade and the Baltic grain trade

may have bulked quantitatively larger in the seventeenth-century Dutch economy, but the Dutch East India Company trade became the model for Europe's commercial revolution: a re-export trade that encouraged free-trade practices in Holland, low interest rates, capital accumulation, easy exchange of securities, and a close partnership between merchants and government. The Asian trade produced similar results in Britain. In addition, the importation of large quantities of cheap Indian cotton goods after 1650 stimulated the innovations in the British textile industry associated with the beginnings of the Industrial Revolution. In fact, Rothermund demonstrates that the Asian trade was also pivotal in the development of free-trade theory. Adam Smith's predecessors were men like Thomas Mun, Charles Davenant, Josiah Child, and the anonymous author of "Considerations upon the East Indian Trade," who defended the British East India Company against its mercantilist opponents. The Asian trade, therefore, was crucially important for the commercial, financial, and industrial revolutions in Europe.

The trade produced no such results in Asia, however, because, Rothermund contends, the major Asian empires during the seventeenth and eighteenth centuries—the Ch'ing dynasty in China, Mughal Empire in India, and Tokugawa Japan—were land-based empires for which the trade with Europe was of peripheral importance and thus attracted little government interest beyond its tax potential. In each of these empires the land tax remained the mainstay of the governments and thus prevented government policy from being influenced by commercial considerations. Even the unfavorable balance of payments characteristic of the Asian trade seemed to benefit Europe. The continuously increasing flow of silver into the Asian lands seems to have reduced the value of silver in Asia, kept its value high in Europe, and allowed Europeans to import massive quantities of Asian goods at remarkably stable prices. Rothermund observes that contemporary economic theory, dominated by equilibrium models, seems unable to explain the phenomenon. He suggests that what happened in the Asian states is analogous to a modern currency devaluation.

It is a small book; I wish it were much larger. It is suggestive rather than exhaustive, but it clarifies and synthesizes the state of research on an exceedingly important topic. It should stimulate some critical discussion and more research. For this reason and because it is useful as it stands for all students of early modern history, it should be translated into English. It contains a good annotated bibliography.

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RAYMOND GREW, editor. *Crises of Political Development in Europe and the United States*. (Studies in Political Development, number 9.) Princeton: Princeton University Press. 1978. Pp. xi, 434. Cloth \$27.50, paper \$6.95.

Most historians may not have noticed—why should they?—but during the 1960s a self-conscious group of political scientists sponsored by the Committee on Comparative Politics of the Social Science Research Council developed schemes for analyzing the “political development” of nations old and new. Broad analytic dimensions of political functioning were elaborated in an attempt to encompass Third World and Western “political systems” within a single universally applicable theoretical framework. All such systems, it was hypothesized, would “modernize” (or had done so) by developing greater political equality, governmental capacity, and differentiation of specialized political institutions. What is more, a set of five basic “crises” was pinpointed, through which all modernizing polities might be supposed to pass: (1) a crisis of identity, or the development of a sense of shared nationality; (2) a crisis of legitimacy, in which the government comes to be accepted as morally valid; (3) a crisis of participation, referring to the involvement of increasing proportions of the citizenry in political affairs; (4) a crisis of penetration, or governmental ability to exert effective controls over territory and aspects of social life; and (5) a crisis of distribution, in which government meets demands for allocations of material benefits and property rights among strata of the citizenry.

Bravely, the Committee on Comparative Politics decided at a certain point to come down from the theoretical clouds and ask an earthbound collection of ten distinguished historians (and historically oriented social scientists) to “apply” political development theory to their countries of specialization: the United Kingdom, Belgium, Scandinavia, the United States, Spain and Portugal, France, Italy, Germany, Russia, and Poland. The committee was especially curious to see how well its ideas would work for the political history of the West, the birthplace of modernity. Skillfully edited by Raymond Grew, *Crises of Political Development in Europe and the United States* is the offspring of this shotgun marriage of grand theory from political science with the evidence of history. Yet, as any reader will notice from the start, this mixed interdisciplinary union has already ended in divorce; indeed, the partners took to quarreling on the very night of the wedding.

Most of the contributors to *Crisis of Political Development* never were comfortable with the concepts they were supposed to apply. As evidenced by Keith Thomas's scathing critique in the opening pages of his lead-off essay on the United Kingdom,

some of the contributors rejected altogether the teleological and elitist assumptions of an approach that sees modernization as “progress” achieved through stabilizing maneuvers by governmental leaders. At a more practical level, the five crises themselves seemed imprecise and difficult to associate with certain historical events or phases rather than others. The historians ended up treating the crises simply as problems that any government might have to face at any point in history. No significant cross-national sequences were established; nor did it turn out that every country had faced—or resolved—severe difficulties in all of the problem areas. Virtually all of the authors concluded that political development theory downplays matters of fundamental explanatory importance, such as the impact of international environments on national sovereignty and domestic conflicts and the impingement of social structures on political processes and institutions.

What value there is in *Crises of Political Development* lies in the parts rather than the book as a whole. Some of the essays are especially skillful, up-to-date surveys of national political histories: Keith Thomas on the United Kingdom, David Bien and Raymond Grew on France, and Walter Pintner on Russia fall under this rubric. Other essays pull together information not readily available; for example, Aristide Zolberg on Belgium and Roman Szporluk on Poland. Here and there, contributors have taken advantage of the freedom allowed by this sort of book to offer fascinating speculations on long-standing historical problems: examples include Thomas on peripheral nationalisms in British history (pp. 46–56 *passim*); Bien and Grew on the old-regime origins of democratic participation in France (pp. 248–49); and Zolberg on the evolution of Belgian national identity (pp. 106–10, 124–29). Finally, all of the essays contain bibliographies of English-language works on the political histories of the countries discussed, and these may prove useful for students and nonspecialists.

Editor Grew concludes his introduction to *Crises of Political Development* with what can be considered a fitting epitaph not only for this book but also for political development theory itself. “A next step,” Grew says, “should be the careful formulation of historical (and therefore not just developmental) problems, followed by the comparison of realities rather than abstractions” (p. 39). To this one can only murmur a heartfelt “amen.”

THEDA SKOCPOL
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LESZEK KOLAKOWSKI. *Main Currents of Marxism: Its Rise, Growth, and Dissolution*. Volume 1, *The Founders*; volume 2, *The Golden Age*; volume 3, *The Breakdown*.

Translated by P. S. FALLA. Oxford: Clarendon Press. 1978. Pp. xiii, 434; viii, 542; xii, 548. \$19.95; \$19.95; \$19.95.

Leszek Kolakowski first came to the attention of Western readers after 1956, when he cast off the Stalinist orthodoxy of his youth to emerge as the most articulate spokesman of Marxist humanism in Poland, if not in the entire Communist world. By 1968, however, when he was dismissed from his professorship of philosophy at the University of Warsaw and sought exile in the West, his commitment to Marxism, humanist or otherwise, had grown increasingly tenuous. In the decade since, which he primarily spent at All Souls's College, Oxford, Kolakowski turned his ambivalence into outright hostility. Appeals by socialists who had earlier been inspired by his libertarian critique of Stalinism, most notably E. P. Thompson's long and impassioned "Open Letter" of 1973, were turned aside with contempt, as Kolakowski recapitulated the familiar "God that failed" syndrome of prior apostates from Marxism. During that period, his newly militant anti-Marxism expressed itself in a number of shorter essays in journals such as *Encounter*, which often relied on religious as well as theoretical and historical arguments (Kolakowski adopting the label "inconsistent atheism" to describe his outlook). But at the same time, he let it be known that he was preparing a more substantial case for his political transformation. *Main Currents of Marxism*, modestly characterized as a "handbook" devoted to the history of Marxism as a doctrine, is the extraordinarily valuable result. In three hefty volumes totaling some fifteen hundred pages of powerfully written text, Kolakowski has attempted nothing less than a synoptic overview of the entire Marxist tradition from its origins to what he sees as its contemporary "breakdown." Drawing on literature in six languages, he summarizes and analyzes the thought and actions of more than a hundred leading figures in or related to that tradition.

As befits a philosopher, Kolakowski begins volume one, *The Founders*, with a long and insightful account of "the origins of dialectic," which he traces back to Neoplatonist soteriology. Arguing that Marx himself was first and foremost a "German philosopher," he dwells on the philosophical dimension of his theory with particular care. The analysis he presents derives, as he freely admits, from Lukács's interpretation in *History and Class Consciousness*, although he vigorously distances himself from Lukács's Leninist politics. Even though little new ground is broken in his discussion, with its familiar distinction between Marx's anthropocentric philosophy of praxis and Engels's scientistic naturalism, the exposition is a model of lucidity and fidelity. In addition to the attention he pays to the philosophi-

cal roots of Marxism, Kolakowski skillfully presents its economic foundations and supplies telling critiques of the labor theory of value and other essential dimensions of the Marxist position. He also spells out the tenets of historical materialism, whose heuristic power he admits but whose dogmatization he deplores. His remarks on the fallacy of relying on the causal power of the economy "in the last resort" are particularly acute. Volume one also includes short sketches of Marx's major competitors among nineteenth-century socialists and a concluding consideration of the links between Marxism and Leninism, a theme that preoccupies Kolakowski throughout the trilogy.

As its title implies, *The Golden Age*, his second volume, portrays the frequently maligned era of the Second International in essentially sympathetic terms. Not yet rigidly dogmatic in doctrine, fragmented into warring sects, or split into theoreticians and practical politicians, Marxism in the years before World War I experienced rapid institutional growth and considerable intellectual distinction. Although he remains faithful to his Lukácsian reading of Marx's own work and disdains the antinomic fissures in Second International theory, Kolakowski nonetheless gives essentially respectful accounts of the major figures of the era. Included among his portraits are familiar faces like Kautsky, Luxemburg, Bernstein, Jaurès, Lafargue, Sorel, Labriola, the Austro-Marxists, and Plekhanov, as well as less well-known ones such as Krzywicki, Kelles-Krauz, and Brzozowski. The last and most intriguing of this Polish trio is credited with being the first Marxist to sense the distance between Marx and Engels on fundamental theoretical issues. In the second half of the volume, Kolakowski turns to Russian Marxism and the rise of Bolshevism. While acknowledging Lenin's political genius and agreeing that 1917 was not a "Bolshevik *coup d'état* but a true revolution of workers and peasants," he nonetheless emphasizes the continuity between Leninist principles and subsequent Stalinist practice. He has little patience with those defenders of Lenin who point to his final qualms about the direction of the revolution and argues instead that Lenin failed to recognize the roots of the dilemma in his own contempt for law, democratic procedure, and theoretical consistency.

Kolakowski carries his insistence on continuity between Leninism and Stalinism through his third volume, which is devoted to what he sees as Marxism's "breakdown." His treatment of Marxism in power devotes more space to the interaction of doctrine and events than does his discussion of the tradition before 1917. Even in this volume, however, he attributes more causal efficacy to ideology than to circumstances in explaining the betrayal of the revolution. Among the more notable of his analyses

are accounts of the purge trials and their ramifications, the permeation of Marxist-Leninist dogmatism into virtually all facets of Soviet culture, and the impact of the communization of Eastern Europe. When it comes to examining the contention that Bolsheviks other than Stalin, most notably Bukharin and Trotsky, would have followed a very different path, Kolakowski argues that the constraints of the Bolshevik model of revolution would have precluded such an outcome. He seems to reserve a special scorn for Trotsky's fate after his fall from power, which he characterizes as that of "only a deposed leader who tried desperately to recover his role, who could not realize that his efforts were vain, and who would not accept responsibility for a state of affairs which he regarded as a strange degeneration, but which was in fact the direct consequence of the principles that . . . he had established as the foundations of socialism."

In the second half of *The Breakdown*, Kolakowski shows himself no more amenable to the proposition that Western Marxism or Maoism offer a significant improvement over the Soviet model. In perhaps the most vituperative and ungenerous chapters of the trilogy, he examines the thought of Lukács, Korsch, Gramsci, the Frankfurt School, Goldmann, and Bloch, while passing more rapidly over Sartre, Althusser, and various East European revisionisms, in a general discussion of Marxism after Stalin's demise. Lukács is characterized as "perhaps the most striking example in the twentieth century of what may be called the betrayal of reason by those whose profession is to use and defend it." Marcuse is damned as the "prophet of semi-romantic anarchism in its most irrational form" and the "ideologist of obscurantism." And Bloch is denounced as a "preacher of intellectual irresponsibility." These and comparable judgments are delivered from the perspective of a thoroughgoing positivism, which stresses the irreconcilable disunity of facts and values and rejects a praxis-grounded definition of truth as mere "species relativism." Relying on Popper's criterion of falsifiability, he dismisses the scientific pretensions of Marxism and concludes that it is best understood as "the greatest fantasy of our century." Attempts at grounding Marxism in nonscientific ways, most importantly that of Habermas, he gives only an impatient glance. In short, no one is likely to quarrel with Kolakowski's own admission that he could not treat the more recent period "with the desirable detachment."

Inevitably, the careful reader can also find the odd factual error or dubious interpretation to challenge, although it is remarkable how few of these there are in so mammoth an undertaking. And with equal certainty, some readers will bemoan the absence of their own favorite "main current," such as

Marxist historiography or the Marxist attitude toward the Jewish question. But however one may wish Kolakowski had come to different conclusions or investigated other issues, there can be no doubt that *Main Currents of Marxism* is an awesome achievement of synthetic scholarship and critical analysis. Ironically, in exorcising his demon, Kolakowski has produced a monument of committed learning whose every page undercuts the positivist assumption that facts and values can indeed be held apart.

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JAMES MILLER. *History and Human Existence: From Marx to Merleau-Ponty*. Berkeley and Los Angeles: University of California Press. 1979. Pp. ix, 287. \$17.50.

James Miller's *History and Human Existence* is a lucid and extremely readable account of rationalist subjectivity and individuation as essential aspects of Marx's thought, the eclipse of these aspects in the objective determinism of orthodox Marxism, their relatively dogmatic retention in a variety of early twentieth-century Marxists (for example, Rosa Luxemburg and György Lukács), and their critical reformulation in the "existential Marxism" of Jean-Paul Sartre and Maurice Merleau-Ponty. Parts of this story have been told before, especially in the cases of orthodox Marxism and Sartre, but Miller's book is well worth reading, for it provides an informed discussion of the pertinent problems in Marx's own work, and it re-emphasizes the importance of Merleau-Ponty. It brings out the one-sidedness of interpretations of Marx that devote exclusive attention either to his "objective," deterministic science of history or to the communitarian elements of his thought that might seem to justify totalitarianism. It also shows how the thought of Sartre and Merleau-Ponty raised questions about Marx's understanding of rationality and freedom—Sartre through his exploration of the anxiety and burdens of freedom and Merleau-Ponty through his rich and problematic interpretation of the ambiguity of history. Seen in more general terms, Miller's study is an excellent example of the recent attempt to reinvigorate critical thought by reassessing the role of subjective agency and individuation in the history of radicalism.

Miller is especially good in tracing the tensions in Sartre's thought, notably the seeming contradiction between a notion of absolute freedom and a notion of situated freedom. He also explores in an extremely suggestive way Merleau-Ponty's vision of ambiguity, its crucial relation to language and institutions, and its implications for an understanding of the historical process. Miller recognizes important

differences between Sartre and Merleau-Ponty, but his own attempt to set forth the basis of existential Marxism in human subjectivity and individuation makes him take those differences less seriously than did Sartre and Merleau-Ponty themselves. Miller has the pieces to carry the debate between these two giants of modern French thought beyond the more immediate political issues that divided them, but he does not put the pieces together in a forceful way. His desire for a synthetic perspective, centered on the active human subject, also leads Miller to underplay somewhat the tensions he sees in Marx's account of subjectivity (for example, in relation to class consciousness) and objectivity (for example, in relation to laws of economic development) as components of critical thought and action. A pointed discussion of "interest" in Marx describes the notion as an ambivalent supplement to seeming opposites such as passion and reason or individual and class. But Miller strains to present this notion as the "heart of Marx's synthesis," reconciling subjectivity and objectivity through a neo-Enlightenment "comprehension of subjectivity as rationally directed interest and causally effective labor" (p. 97). An approach to interpretation with a less one-sided interest in synthesis might have enabled Miller to explore more fully the interaction between unifying and self-contestatory movements in Marx, and the results might not have been altogether negative.

One important question that Miller does not adequately address is whether the attempt to provide a historical and critical account of what is living and what is dead in Marxism can enter into a constructive dialogue with recent French thought if it confines itself to selected aspects of existential Marxism. Miller understandably dismisses the heavy-handed, tendentious attack upon subjectivity of Louis Althusser, who equates the critique of humanism with an often confused defense of "scientific" objectivism. But Miller neglects the more cogent and nuanced critiques of humanism and the subject to be found in a number of recent French figures (Jacques Derrida, Philippe Sollers, Michel Foucault, and Gilles Deleuze among others). These figures might have forced Miller to reconsider some of his own assumptions about interpretation, for they have focused attention upon the problem of the relationship between the desire for binding synthesis and disseminating tendencies that contest it. In the process, they have provided rather different interpretations of Nietzsche and Heidegger from Miller's own.

With only relatively marginal doubts, Miller fits the historical process he treats into too neat a pattern: the achievement of Marx's synthesis, the breakdown of the synthesis in orthodox Marxism, its often doctrinaire recapitulation in more or less idealist forms of Marxism, and its primarily con-

structive re-creation in existential Marxism. This historical process was, I think, both more intricate and somewhat different in nature. (An elaboration of Miller's own brief discussion of Gramsci, Luxemburg, and the Critical Theorists would itself alone complicate the story considerably, and it is difficult to reconcile his often critical analyses of particular works by Sartre, especially the *Critique of Dialectical Reason*, with his favorable general estimate of Sartre's contribution to existential Marxism.) Yet, despite the objections one may make to the overall cast he gives to his account, one must readily recognize Miller's study as a significant contribution to the ongoing attempt to come to terms with the Marxist tradition. It is consistently informed by a fine intelligence addressing itself to important problems, and many of its specific interpretations are cogent and knowledgeable.

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A. J. H. LATHAM. *The International Economy and the Underdeveloped World, 1865-1914*. Totowa, N.J.: Rowman and Littlefield, and Croom Helm, London. 1978. Pp. 217. \$21.50.

As secretary to the Third World Group of Economic Historians, A. J. H. Latham's primary purpose is to correct the view that the world economy of the period 1865-1914 was chiefly a British and a North American phenomenon. On the contrary, says the author, the world economy would never have developed as it did without the indispensable contribution of the poorer parts of the world.

In order to obtain "a truly global view of what the world's economy really was" (p. 13), Latham has provided us with several essays concerning communications, money and capital, international trade, population and migration, growth and fluctuations, social responses, and economic development and social change. Alas, the underdeveloped world being as large as it is (just how large is not clear) and the book being as slim as it is, the author has had to restrict himself to dealing with certain aspects of Asian and African history. For those parts of the world that are covered, there is ample statistical data to support the quantitative approach adopted here.

Although this work provides us with an informative and stimulating discussion of the world economy in the fifty years before 1914, it does so (partly out of necessity where the canvas is so large) within the confines of existing themes and with the use of existing materials. The student and the non-specialist may find a good deal that is new here, but for the international economist and economic historian this book holds few surprises. Nor are the hopes

aroused in the introduction, concerning the development of the world's railways and world freight rates, fully realized. There is, in fact, little here that has not already appeared in print—not least, in my own *Impact of Western Man* (1967) and my *Emergence of an International Economy* (1970). Moreover, while the author is to be commended for criticizing those who see the development of the world economy in Western terms alone, the idea that the West is not the world long precedes these pages. The late A. J. Toynbee, L. S. Stavrianos, Geoffrey Barraclough, and I—among others—have been saying this for years.

Indeed, my worry is whether the author does not press his point concerning the role of the underdeveloped world a bit hard. His assertions that Britain's free-trade policy was dependent on its Asian balances, or that the Africans and Asians were equally able to respond to economic incentives, I find difficult to accept. The point about economic incentives, surely, is not that individual Africans and Asians (under constraint) could be as capitalistic and money and market oriented as any Westerner, but whether these societies were as committed to these values as Western societies were.

Ultimately, the worth of any book depends on what use is made of it. I would think that the best use that could be made of this work would be to employ it as a text or as supplementary reading, and for this purpose it would have my support.

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TREVOR I. WILLIAMS, editor. *A History of Technology*. Volume 6, *The Twentieth Century c. 1900 to c. 1950*, part 1; volume 7, *The Twentieth Century c. 1900 to c. 1950*, part 2. Oxford: Clarendon Press. 1978. Pp. xxv, 689; xix, 692–1530. \$82.00 the set.

During the 1950s the history of technology assumed the institutional features of a distinctive historical specialty complete with a society, a journal, university courses, and graduate curricula. The new discipline was also fortunate that in these same years a team of British scholars surveyed the entire subject from prehistory to 1900 in five monumental volumes entitled *A History of Technology* (1954–58). Its chief editor, Charles Singer, had conceived the project and subsequently recruited three associate editors: E. J. Holmyard, A. R. Hall, and Trevor I. Williams.

To make the project manageable, the editors defined technology fairly narrowly and confined the narrative largely to the West. Believing that twentieth-century technology was too diverse and complex for communication to nonspecialists, they chose 1900 as the cutoff date. They also felt it to be too recent for discerning historical analysis.

Despite these reservations, one of these editors, Trevor I. Williams, decided some twenty years later to carry the narrative forward to 1950. The passage of time had provided a surer perspective, and the unexpectedly vigorous sale of the initial volumes encouraged him to take up the challenge of making the development of recent technology comprehensible to laymen. He has succeeded to a high degree, thereby assuring that the whole set, now augmented by two volumes, will remain in the foreseeable future the most comprehensive treatment of the subject, an indispensable reference work for any academic or public library.

Some fifty-eight chapters by as many authors treat almost every conceivable technical topic plus a few, such as the technology of ocean depths, which might not come readily to mind. Chapters two through seven on the symbiosis of modern technology with science, business, trade unions, government, and education are excellent. Nothing quite like them was possible when the earlier Singer volumes were written. Yet, although they reflect a quarter century of expanding scholarship in the history of technology, they do not do so to the fullest extent. Most curious, for instance, is the absence of a discussion of the growth of the engineering professions. Also, I believe that an introductory chapter on the historiography of technology in the twentieth century would have served the reader better than the existing one, which reviews political history since 1900 with no attempt at integrating technology with general history.

The remaining fifty-one chapters describing the progress of various technologies are all technically competent. Many are fascinating, but their historical sophistication varies widely. Although the chronology is usually clear, most chapters are Whiggish and betray a protechnical bias that soft-pedals the effects of modern military technology and occasionally erupts into open contempt for environmental critics (pp. 256, 1481). Still, there are several examples of outstanding historical craftsmanship. My favorite is Lynwood Bryant's history of the internal combustion engine, which blends high erudition, both technical and historical, with keen observation on the obvious and subtle ways these motors have transformed society.

The illustrations and charts are very useful. One wishes for more, but space and cost considerations probably made this impractical. Much less excusable is the documentation. Footnotes and bibliographies are frequently skimpy, in some cases altogether absent. The bias is distinctly Anglo-Saxon: British technology receives primary attention; the United States comes next; Western Europe and Japan are a weak third; Eastern Europe is covered even more lightly; and the rest of the world is hardly mentioned—Latin America, not at all. Yet

this history of twentieth-century technology is the most competent and comprehensive available. It challenges other societies to do likewise from their cultural perspective. To do it better would be quite an achievement.

JOHN J. BEER
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THEDA SKOCPOL. *States and Social Revolutions: A Comparative Analysis of France, Russia, and China*. New York: Cambridge University Press. 1979. Pp. xvii, 407. Cloth \$29.50, paper \$7.95.

This is a clear, concise, and admirably well-crafted book. The argument may be summarized in Theda Skocpol's own words as follows: "The revolutionary crises developed when the old-regime states became unable to meet the challenges of evolving international situations. Monarchical authorities were subjected to new threats or to intensified competition from more economically developed powers abroad. And they were constrained or checked in their responses by the institutionalized relations of the autocratic state organizations to the landed upper classes and the agrarian economies. Caught in cross-pressures between domestic class structures and international exigencies, the autocracies and their centralized administrations and armies broke apart, opening the way for social-revolutionary transformations spearheaded by revolts from below" (p. 47). And, "Questions of state power have been basic in social-revolutionary transformations, but state power cannot be understood only as an instrument of class domination, nor can changes in state structures be explained primarily in terms of class conflicts. In France, Russia, and China, class conflicts—especially between peasants and landlords—were pivotal during the revolutionary interregnums. But the nature of the New Regimes that emerged from the revolutionary conflicts depended fundamentally upon the structure of the state organizations and their partially autonomous and dynamic relationships to domestic class and political forces, as well as their positions in relation to states abroad" (p. 284).

Politics, in short, comes first. Society and the economy may set limits but do not define the course nor determine the result of social revolutions. The state is not reducible to an expression of class interest, but has interests and a dynamic of its own. Competition between tax and rent collectors for peasant resources may, in fact, be taken for granted in Old-Regime, predominantly agrarian societies. State interests and the interests of the predominant social class are therefore systematically opposed, save when both are threatened by some foreign power. If such a threat fails to provoke reconcilia-

tion of the contending interests of rulers and landed classes, social revolution has its chance.

Skocpol illustrates her general ideas by analyzing the French, Russian, and Chinese revolutions in some detail, finding a basic parallel in the causes of each revolution, even though their outcomes diverged significantly and systematically from case to case. Along the way she scatters a series of arresting summary statements. For example: "To make his eclectic system work, Napoleon judiciously dispensed with nonroutinized mass mobilizations and with all manifestations of ideological commitment. Wielding instead symbols, rituals, and propaganda of a highly generalized French nationalism, Bonaparte decorated his essentially authoritarian-bureaucratic regime with a hodgepodge of symbolic concessions to the inherited factions: plebiscitary and patriotic rituals for the radicals; consultative councils with restricted electoral bases for the liberals; and a Concordat with the Catholic Church for conservatives" (p. 195). Shrewd and persuasive comparisons punctuate her analysis of the separate revolutionary processes. I particularly liked her argument about why the Bolsheviks were able to hold power whereas the Montagnards were not (p. 192) and the discussion of divergent relations of Russian and Chinese Communists to the peasantry of their respective countries (pp. 277-79).

With so much to admire, it is perhaps captious to complain about Skocpol's implicit repudiation of the role of personalities in affecting the revolutionary processes she analyzes. To allow that sort of variable would, perhaps, spoil the sociology she seeks to discover in human affairs. She also clings to a residual revolutionary romanticism, as her final pages demonstrate, even though the logic of her argument makes Marx's aspiration for "working-class-based socialism" entirely implausible so far as I can see, though she declares it a "necessity" (p. 292).

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DAVID BIALE. *Gershom Scholem: Kabbalah and Counter-History*. Cambridge: Harvard University Press. 1979. Pp. vi, 279. \$16.50.

This well-conceived and impressively executed study of Gershom Scholem as a historian deepens our appreciation and extends the importance of this pioneering and still leading scholar of Jewish mysticism. David Biale shows how Scholem uses history as a discipline for determining indigenous Jewish sources and thus for preserving the legitimate tradition of Judaism. He also shows how Scholem's use of history is itself a part of that legitimate tradition—that is, one more application of the interpretive mode of Kabbalah, the esoteric Jewish mys-

tical tradition of documentary reinterpretation. Yet Biale, who otherwise closely identifies with Scholem, skillfully shows that Kabbalah and Scholem's study of Kabbalah also provide a structural model for revitalizing modern historiography. In this, Biale goes beyond Scholem's intent but for laudable purposes.

In the most erudite and convincing parts of his study, Biale argues that Scholem's youthful return to Judaism at the turn of the century was among the most radical Jewish responses to the crisis of bankrupt assimilation. Motivated less by anti-Semitism than by filial dissatisfaction with an apologetic accommodation of Judaism to rational and ethical ideals, and finally rejecting as equally assimilative the attempts of his contemporaries to reconstruct their ties to Judaism (notably, Martin Buber's pre-war Nietzschean *Erlebnismystik* and the Zionist program of militant nationalism), Scholem struck out on his own "idiosyncratic" course to restore Judaism to its indigenous tradition.

As Biale makes clear, Scholem found in Kabbalah the most genuine and vital expression of Judaism. In describing Scholem's discovery of resurgent religious vitality in Jewish mysticism, Biale makes too little reference to the content of Kabbalah, a limitation which not only obscures the reason for Scholem's enthusiasm for Kabbalah, but produces a difficult stylistic elusiveness. Biale's strength lies instead in describing Scholem's historiographical argument for restoring Kabbalah to the center of Judaism. Two of Scholem's arguments are especially well represented in this study: the distinct immunity of the secret Kabbalah from secular influences and its consequent capacity to rejuvenate the normative though errant philosophico-legal tradition of rabbinic Judaism.

Ultimately, Biale argues that, by bringing to light the sources as well as the Sabbatian and Hasidic legacies of the subterranean Kabbalistic Jewish tradition, Scholem not only created a major revision of Jewish historiography but also contributed to constructing a new historiography. Though the new historiography bears a structural resemblance to Kabbalah, Biale prefers to call the unapologetic highlighting of previously suppressed material "counter-history." Counter-history clearly differs from "mainstream" historiography and even from revisionist historiography, which proposes a new philosophy of history or finds new facts. Yet, in spite of Biale's suggestion to the contrary, counter-history does not seem to escape the typical pitfalls that preclude historical objectivity. For example, Scholem's lifelong "hunt for [the] vital element" of irrationalism in Judaism (p. 201), which Biale himself characterizes as "a steady attempt to substantiate certain extraordinary intuitions as a student" (p. 133), is difficult to reconcile with the

critical method of constantly modifying *a priori* hypotheses under the impact of the sources, a process that forcibly redirects, not merely refines, the original investigation of the sources.

Nevertheless, counter-history serves as a useful framework for defining the structure of Scholem's prodigious penetration of the Jewish sources from the interpretive layers of mythmaking to the alien, crystalline past. By bringing to light one of the major thinkers of the twentieth century, Biale has provided an important study, one that will stimulate discussion among Jewish and European historians as well as among philosophers of history.

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WILLIAM A. CLEBSCH. *Christianity in European History*. New York: Oxford University Press. 1979. Pp. xii, 315. Cloth \$14.95, paper \$4.50.

William Anthony Clebsch in about 280 pages has drawn together a fresh synthesis of the history of Christianity in European culture. Thoughtful and knowledgeable about church history, Clebsch concentrates on human responses to manifestations of Christ in history, and thereby attempts to combine the partial insights of cultural and church history by reference to a human-centered history of religion. He accomplishes his purpose by selecting illuminating cases of varied human responses to manifestations of Christ. After setting the stage for such manifestations in European culture, Clebsch divides the history of Christianity into five main periods, which he labels citizenship, chaos, unity, allegiance, and autonomy, divided chronologically by Constantine, Charlemagne, Innocent III, and Louis XIV. As sovereignty shifted, according to Clebsch, Christianity served as an agent for European civilization in varied ways.

Within each of the five periods, Clebsch selects two religious exemplars of Christian forms of humanity in cultural transition. These are martyrs (Perpetua) and monks (Anthony) in the first period, theodocists (Boethius) and prelates (Gregory the Great) in the period of chaos, mystics (Bernard of Clairvaux) and theologians (Anselm of Canterbury) in the unity period, moralists (Jeremy Taylor) and pietists (Zinzendorf) in the period of allegiance, and in the recent autonomy period Clebsch selects Bonhoeffer as an activist representative and waffles between Newman, Kierkegaard, and Ritschl as apologist representatives before finally selecting Kierkegaard.

Clebsch notes that traditional interpretations favor his martyr-monk and pietist-moralist categories, while theodicy-prelacy and activist-apologist rubrics

are less well supported by traditional historiographies. At any rate, Clebsch's method provides the reader with an exemplary teaching tool and a suggestive organizational scheme. The strengths of his book are his willingness to marshal materials under a clear interpretative scheme and his frequent ability to make use of unhackneyed materials. The level of learning throughout is high.

On the negative side, Clebsch's interesting book is likely to fall between two stools. While Clebsch may put off theologically oriented historians by a bit of disdain for their doctrinal commitments, his obvious enthusiasm for the effects of the "various manifestations of Christ" in history may offend his more secular historical colleagues.

The reviewer appreciates Clebsch's willingness to give credit to the claims of faithful Christians for being what the believers said they were—a perspective impenetrable to many modern sophisticates. Clebsch does neglect some of the greatest figures of Christian history, including Augustine of Hippo, the reformers, and Eastern Orthodox leaders except Justinian. He is weak on Scriptures and ecclesiology, and he appears to be enamored with what the most recent period has contributed to the understanding of Christianity. What Clebsch has done, however, he has done well.

Clebsch's presuppositional method deserves further discussion, development, and restatement. On first reading, it appears to be facile and insufficiently grounded methodologically. But it has the advantage of middle-ground, not too erudite characteristics that might possibly bridge gaps and establish connections among historians, sociologists, and theologians, who sometimes find difficulty in speaking and working together.

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KENNETH LEVIN. *Freud's Early Psychology of the Neuroses: A Historical Perspective*. Pittsburgh: University of Pittsburgh Press. 1978. Pp. ix, 314. \$12.95.

Kenneth Levin's monograph is a detailed account of the development of Freud's earliest psychoanalytic theories and of interest only to those already deeply involved in psychoanalytic history. By no means could it serve as a primer or an introduction for students. It is a book whose primary theses I find unsubstantiated, a work with many faults, and yet it contains some truly fine sections on the history and themes of nineteenth-century medicine, psychiatry, and neurology.

Levin argues two major positions. The first is that Freud, from the very start of his work with neurotic patients (ca. 1886), offered only psychological explanations and eschewed physiological theories of neurosis. Levin's second theme is that Freud's psy-

chological theories were not influenced by his early neurophysiological training and experience. I am at a loss to understand why Levin espouses two such extreme positions. His own discussions of Freud's theory-building even show that, at first, Freud strongly supported Charcot's physiological theories about hysteria and hypnosis and also that Freud believed neurasthenia and anxiety had physiological etiologies. Even stranger is Levin's insistence on totally negating the abundant evidence that Freud's neurophysiological training influenced his conceptualization of psychological mechanisms. Levin is committed to making an alternate case for the significance of Freud's clinical experience in his development of psychoanalysis. This is an important point (though hardly a novel one), but it does not justify the lopsided assertion that years of physiological research and experience made no impression on Freud. Levin could have written a much finer book if he had not felt compelled to mold his study around tenuous theoretical constructs, which often force him to distort his evidence to fit their rigid lines.

Levin is an able scholar when it comes to presenting the medical and psychiatric themes and controversies of Freud's day. He has accumulated a wide variety of information, especially on the struggle between the anatomists and physiologists over the etiology of disease, and he has a talent for explaining clearly the essence and significance of the thought of various scientists. He elucidates neatly the controversy surrounding Freud's 1886 paper on male hysteria as well as the enmity that developed between Meynert and Freud.

Unfortunately, more yet is amiss with Levin's monograph. It is a doctoral dissertation evidently completed in 1973 and published in 1978 with no attempt to incorporate recent scholarship. The references end at 1972, and the book suffers accordingly. The most glaring omission is Pribram and Gill's *Freud's "Project" Re-assessed* (1976). Even Levin's doctoral research is not complete. He should have consulted David Rapaport's *The Structure of Psychoanalytic Theory* (1960) and Stephen Kern's 1970 Ph.D. study, "Freud and the Emergence of Child Psychology, 1880-1910," a work that covers some of the same medical-historical ground as Levin's monograph. Levin's consistent misspelling of Salpêtrière is annoying, as are his overly long sentences (up to eighty-two words). He is given to broad statements, either oversimplifying a complex situation or making assertions without proof. These shortcomings, as well as Levin's one-sided and intrusive theorizing, mar his lucid account of certain late nineteenth-century medical and psychiatric developments and his well-organized presentation of Freud's early writings.

HANNAH S. DECKER

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VERN L. BULLOUGH and BONNIE BULLOUGH. *Sin, Sickness, & Sanity: A History of Sexual Attitudes*. New York: Garland Publishing, 1977. Pp. ix, 276. \$13.95.

There has long been a need for a short, concise, and readable history of sexuality not only for the general reader but also for undergraduate courses that deal with the topic. Vern L. Bullough and Bonnie Bullough have filled the gap by providing us with a volume that successfully draws on Vern Bullough's larger study, *Sexual Variance in Society and History* (1976). The new book has the strengths and weaknesses of the longer study. There are few, if any, scholars who know as much about the history of sexuality as Vern Bullough. His knowledge is encyclopedic, and it has been effectively distilled into this general history. The Bulloughs also are able to bring in a comparative perspective that will be very useful for the general reader. While the larger study gives us much more, there is some material in this volume on how the Western experience differs from Islamic, Taoist, and Hindu attitudes toward sexuality.

The Bulloughs study, however, has some shortcomings. Christianity is defined as a sex-negative religion, and there is great emphasis on the origins of antisex attitudes in Greek philosophy. After that, according to the Bulloughs, not much happens until the nineteenth and twentieth centuries. Differences from age to age and place to place shrink in comparison to what happened when Christianity was young. The subtle and not so subtle distinctions that are so important in history are ironed out. Origins are all; development and change very little. Readers of the Bulloughs earlier study on women in history will recall the same tendency. Women have always been the subordinate sex, and this single fact drowns the differences in the position of women in time and space.

A second difficulty lies in the very kind of history the Bulloughs write. Because they deal with attitudes rather than practices, one cannot help but wonder if their study has not confused "what is and what ought to be," to borrow Carl Degler's phrase. There are a growing number of studies that examine sexual practices by drawing on legal and church records. These studies show that under the blanket of two thousand years of purported sex negativism, there was remarkable variety. Studying the pronouncements of Church fathers and nineteenth-century doctors does not take us very far in penetrating into temporal and spatial distinctions. A study of these differences raises a whole host of questions barely touched by the Bulloughs. Explaining these differences takes us out of the world of ideas into the social, political, and economic permutations that account for sexual change.

In addition, concentration on attitude leads the Bulloughs to overemphasize the ambiguity of so-

domy and masturbation in the West. It is true that studying those few writers who commented on sodomy leads to the conclusion that there was no clear-cut view on precisely what these sinners did in their sexually deviant careers. A study of legal *practice* helps to lift the veil of obscurity. On masturbation it seems reasonable to assume that those medical men who not only wrote about but *prescribed* cures for the masturbator knew precisely what they meant by the *solitary* vice and *self-abuse*.

One should not be too hard on the Bulloughs for not writing a different book. This is a well-done and useful volume. My comments should serve as a reminder that there is more to the history of sexuality than attitude studies and a great deal more of largely unexplored richness and variety in the Western experience than the Bulloughs' study indicates.

ARTHUR N. GILBERT
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TAMARA K. HAREVEN, editor. *Transitions: The Family and the Life Course in Historical Perspective*. (Studies in Social Discontinuity.) New York: Academic Press, 1978. Pp. xx, 304. \$19.50.

Unfortunately, this book will not persuade anyone that, as one of the contributors claims, statistics "offer us a richer and more accurate understanding of the social history of the family than has hitherto been available" (p. 66). On the contrary and as the contributors admit, it presents us with conclusions already well known. The result of "workshops" directed by Tamara Hareven and sponsored by the Mathematics Social Science Board, *Transitions* is an expression of that modern movement aiming to make the study of human life appear scientific by presenting its evidence in numerical form.

The "exposure" of Hareven's groups to Glen Elder's notion of the "life course" revealed to them that the idea of family life as a series of stages did not do justice to change. While this might seem obvious to the uninitiated, Elder and his followers are not daunted and have managed to mystify it in deadly sociologese. Four of the chapters, on marriage, school attendance, women and work, and old age, address family "transitions," basing their conclusions on the United States 1880 census of eight Massachusetts towns. In other chapters, Elder provides the common theme, Peter Uhlenberg addresses the different experiences of birth cohorts since 1879, John Modell and Howard Chudacoff briefly describe the Essex county "setting," Modell and Hareven compare preceding chapters to Elder's notion, Stanley Engerman tells us how useful to history is the theory of "budget restraint," and Robert LeVine describes the "life course" of the Gusii of Kenya. The authors confess to the profound weakness of basing their analyses on a single date. This is

particularly ironical given their desire to look at change rather than "static snapshots" (p. xix). The best chapter is by Carl A. Kaestle and Maris Vinovskis on school attendance, in large part because it draws its evidence from two points in time.

The book's faults are characteristic of its genre. It assumes that statistical coincidences add up to historical explanation. Although it ignores or denigrates nonstatistical evidence, even calling it "gossip" at one point, *Transitions* depends on it. It accepts the functionalist view that the family is simply an agent of socialization and, as a corollary, it presents "social discontinuity" invidiously, as "deviant sequences" and "faulty socialization." With such a world-view, one wonders how social change can ever be understood. The same attitude fosters the invidious view of women one finds in much historical demography, where their only useful function is viewed as reproduction; here Uhlenberg writes of a "hardcore of 'unmarriageable' females" (p. 85). The statistical superstructure rests on a ludicrously simple idea of motive whereby people in the past calculate suspiciously like the historians describing them. "Individuals weigh the benefits of acting now rather than later by considering the nature of the present state as against the quality of the future state, discounted for the uncertainties of attaining the desired state in the future" (p. 247). Hareven and her co-authors choose "behavioral data" and largely ignore "phenomenological data" even though they recognize that this makes it impossible "to reconstruct the meaning to people" of "life course decisions" (p. 14). They cite each other and ignore the relevant work of other kinds of historians. They describe no individuals, even though they refer frequently to "individual decisions." Finally, the language in which they present their findings is crippling to their subjects as well as to themselves. It is a moot point whether it is better to neglect the lives of ordinary people as they were neglected before the advent of the "new social history" or to represent them, as these historians do, in the dehumanized form of income-consumption percentages.

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Daedalus: Journal of the American Academy of Arts and Sciences. Volume 107, number 4, *Generations*. Cambridge: American Academy of Arts and Sciences. 1978. Pp. vii, 207. \$4.00.

A generation—the ordinary period of time required for children to succeed parents—used to be counted as a third of a century or about thirty years. Now, although parents with children in college or gradu-

ate school may find it hard to believe, its length is supposed to be twenty years. In the future, given the precocity of youth and American preference for the new, the span of a generation may shrink to a decade. We already use the word generation not only to denote a period of years but also to characterize a cohort of people born around the same time. In one of the essays included in the twentieth anniversary issue of *Daedalus*, Noel Annan reminds us that it is the intelligentsia who define a generation as "lost" or "beat" and that they do so mainly in terms of their own ideas, tastes, and behavior.

The thirteen contributors to *Generations* were born between 1896 and 1948, all but four before 1930, and only two after World War II. The oldest, Douglas Bush, acknowledges that he feels "something of a stranger in the present world" (p. 165). Noel Annan, born in 1916, writes as a member of an aging generation, once the hunter of old fogies, now the prey of young achievers. On the other hand, Jonathan Lear, in an article on two generations of leading positivist philosophers, addresses himself to and seems to consider himself one of the current generation of young nonpositivist philosophers. Laura Nash (born in 1948), with Lear the youngest of the contributors, concludes her essay on Greek origins of generational thought with the observation that in an age when people refuse to have children and talk more about generations of computers than generations of mankind "generation has lost its reference point" (p. 19).

One of the advantages and intentions of collections like this is to acquaint readers with the concerns, methods, and vocabularies of students working in different disciplines but sharing a common allegiance to humanistic culture. Contributors to *Generations* represent the fields of history, anthropology, sociology, political science, literature, classics, and philosophy. The essays, grouped under the headings "The Idea of Generations," "Generations in Historical Perspective," and "Generations in Recent Scholarship," range from Harold R. Isaac's "Bringing Up the Father Question," described by the author as a "question-raising exercise" (p. 202), to Matilda White Riley's more formal and expository "Aging, Social Change, and the Power of Ideas." Historians will probably be most interested in three articles in the middle section, each of which includes the word "Reflections" in the title: Annan reflects on three generations of twentieth-century England, particularly the achievements and foibles of the interwar generation; Carl Schorske presents a concise and thoughtful summation of his studies of cultural innovation and generational tension in late nineteenth- and early twentieth-century Vienna; and Morton Keller, in a temperate, undogmatic way, explores the role of intragenerational similarity and intergenerational change and conflict

on American political development. "It may be," he states at the outset of his analysis, "that generations play in American politics a role not unlike that of classes in European political life" (p. 123).

The most reassuring note in the volume comes at the end of Shirley Robin Letwin's article on the treatment of generational differences in Trollope's novels. In Trollope's world "a gentleman is immune to the vulgar error of elevating every disagreement or attempt at innovation into a battle between the forces of good and evil. He assumes that a difference in judgement or taste can be indulged without making a revolution" (p. 70). Let's hope this applies to gentlemen, and ladies too, in our own world.

ROBERT H. BREMNER
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JOSÉ MARÍA ALEGRE. *Las relaciones hispano-danesas en la primera mitad del siglo XVIII*. (Études Romanes de l'Université de Copenhague. Revue Romane, special issue 14.) Copenhagen: Akademisk Forlag. 1978. Pp. 534.

The title of this well-researched study is misleading in that the contents are almost exclusively concerned with a decade of diplomatic relations between Madrid and Copenhagen during the War of the Austrian Succession. José María Alegre, a professor in the Institute of Romanic Studies at the University of Copenhagen, has carefully sifted the relevant archival resources in Spain and Denmark to document a story of ambitious opportunism at the two courts during the 1740s.

After Denmark reopened its embassy in Madrid in 1740 its representatives were urged to seek three major commercial advantages: a greater freedom for Danish vessels in the Caribbean trade, a reduction of tariffs on its Baltic commodities in Iberian ports, and Spanish naval protection for Danish shipping against mounting losses to North African pirates. The court of Philip V at first welcomed the return of the Danish embassy out of its own strategic considerations. It was anxious to prevent Denmark from joining England in the year-old War of Jenkins's Ear against Spain in the Atlantic and the Caribbean, and, in keeping with its close collaboration in French diplomatic aims, Spain desired Denmark to be a buffer against English entry into the Baltic area. On the basis of this mutual interest, both courts seriously studied previous treaties with other powers to compose a draft text for final negotiation. Alegre presents a lengthy comparative analysis of the twelve treaties used by the Danes and the five preferred by the Spanish to establish their objectives.

Early in 1742, however, new and compelling reasons for a Spanish agreement to Danish commercial

ambitions emerged in the outcome of the first phase of the War of the Austrian Succession, as well as in the strong dynastic hopes of Elizabeth Farnese, the wife of the feeble Philip V. Fearing that Denmark might abandon the Bourbon courts and enter a partnership with their enemies (such as the Treaty of Breslau), both Paris and Madrid promised many commercial concessions. For Spain the price was ruinously high in that its historic Caribbean policy would be in jeopardy. Furthermore, the author has shown that Elizabeth Farnese, through her obsequious favorite José Campillo, drafted secret articles for Philip's signature in which even more would be conceded to the Danes. This sacrifice of Spanish overseas interests was planned so that the Danes would remain anti-Austrian during the war. With Maria Theresa's forces engaged elsewhere, Elizabeth planned that a Spanish army could fight more successfully in north Italy for the claims of her children. Without the knowledge or approval of the Spanish foreign secretary, Villarias, her diplomatic gamble was written into the Treaty of San Ildefonso of July 1742. The sudden death of Campillo in April 1743 ended this bizarre intrigue since Ensenada, Carvajal, and Wall, sensitive to Spain's long-term interests, delayed with excuses any actual observance of the treaty. Subsequently the two monarchies again drifted apart.

This meticulous study provides an unfamiliar perspective into the conduct of Spanish and Danish diplomacy in the mid-eighteenth century. The author promises a continuation of his work, which, while leaving unchanged the general outline of the international relations of the century, accords the lesser powers a new and overdue appraisal.

ALBERT J. LOOMIE
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NANCY NICHOLS BARKER. *The French Experience in Mexico, 1821-1861: A History of Constant Misunderstanding*. Chapel Hill: University of North Carolina Press. 1979. Pp. xv, 264. \$18.00.

Nancy Nichols Barker's *Distaff Diplomacy: The Empress Eugénie and the Foreign Policy of the Second Empire* and her two volumes of *The French Legation in Texas* have earned prestigious awards. The former work is already a part of the standard, classic literature on the reign of Napoleon III. It is therefore no reflection on the quality of the author's research or writing to say that her newest book is of comparatively little consequence. Franco-Mexican relations from 1821 to 1861 suffered from a lack of meaningful continuity. We are left therefore with a series of interesting narratives suitable for specialized journals but lacking the unity that justifies a book. Many of the most valuable contributions of this work have

already appeared in the author's journal articles on such French diplomats as Dubois de Saligny.

Students of the subject will, however, acknowledge their indebtedness to Barker for her original and sound research on the demography of the French in Mexico, 1821–61. She has made innovative and sound use of archival and newspaper sources to describe the class structure, economic behavior, and social attitudes of that population. We also learn much of the logistics of communication and travel. The author's archival research is impressive. It includes, besides the correspondence of the French and Mexican ministries of foreign affairs, the gold mine of materials in the archives of the Mexican embassy at Paris. Accompanying these rich collections are Austrian archival materials at Vienna and at Washington, as well as the personal papers and career dossiers of French and Mexican diplomats. British diplomatic correspondence, available on microfilm in this country, is also used. The only glaring gap in an otherwise complete research effort, is the absence of United States diplomatic correspondence. Why should we trust a British diplomat's quotation of an alleged report by a United States minister to the state department (p. 159)? The reader questions how a British diplomat got hold of an American dispatch that could only be damaging to American interests if revealed to the world. The author's bibliography of published secondary literature is unbalanced and highly selective. Important new research and classic works have been omitted, while peripherally important or irrelevant works are included.

A little more attention could have been given to organization. For example, Barker does an excellent job in relating developments throughout the world to French intervention or nonintervention in Mexico, but she sometimes fails to clarify their concurrent interrelationship with events in Mexico that are described fully on other pages.

At a petty level, the reader may become irritated by the practice of putting explanatory material into parentheses thrust into the text. This goes so far as to include translations of French phrases. Still, the author has a delightful sense of humor that makes her most parentheses-laden pages readable. We are left to assume, however, that she is pulling our leg when she states that prior to 1849 "Mexico had usually armed itself in Great Britain (from the Tower of London), buying . . . obsolete models and in small lots" (p. 120).

In addition to the original conclusions drawn from her previous books and journal articles, the most important contributions of this work concern French interest in Tehuantepec, the circumstances of the "Pastry War of 1838," and the evolution of schemes to enthrone various European princes in Mexico prior to the reign of Emperor Maximilian (1864–67).

The last short chapter entitled "In Retrospect" could have been omitted. It indulges in a great deal of sociological speculation concerning the racist motives of nineteenth-century Frenchmen, which are relevant only in twentieth-century terms. The utterly mistaken views of Karl Marx are also explored, though he had no access to the truth about Franco-Mexican relations and drew false conclusions from the little that he did know. Why was he not consigned to a footnote? With allowance for flights of poetic license, it stretches things a bit to describe Emperor Maximilian as "the last of the *conquistadores*" while neglecting to describe the firm and courageous way in which he blocked French schemes to establish a protectorate over Sonora. A book covering the years 1821–61 might better have avoided the subject.

ARNOLD BLUMBERG
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FRITZ VAN BRIESSEN. *Grundzüge der deutsch-chinesischen Beziehungen*. (Grundzüge, number 32.) Darmstadt: Wissenschaftliche Buchgesellschaft. 1977. Pp. xi, 206.

Fritz van Briessen's short book outlines German-Chinese relations from the seventeenth century to the present, paying primary attention to diplomatic contacts in the late nineteenth and twentieth centuries. Van Briessen, for many years a journalist in the Far East, later an official of the West German embassy in Tokyo, and an author of popular books on oriental art, displays the effects of his background in the presentation of his subject. He assumes the primacy of *Aussenpolitik* and connects the formulation of foreign policies to the nature of countries' cultural development and interaction in the traditional style of German diplomacy and historiography. This gives van Briessen's analysis a somewhat old-fashioned sound from the standpoint of recent historians' concentration on the internal political roots of foreign policy. Thus, for example, van Briessen sees the ending of the German imperial presence in China at Versailles in 1919 as a "crystallization point" for the process that led to the radical political changes in China during the following decades. His journalistic background is evident in his tendency to emphasize the formal, publicized events of Sino-German affairs rather than long-term processes of social and economic change, which he treats only cursorily. Certain events of particular interest to a German "old China hand" but of limited historical significance, such as the "war crimes" trial of German agents in China in 1946–47, are given more attention than is probably necessary in so short a book.

The book's main strength is the way it ties together material from disparate sources on a rela-

tionship of secondary importance to both China and Germany. It is highly readable, if occasionally repetitive, and it contains excellent references to the limited published work available on various aspects of the subject. Van Briessen handles the events leading to the seizure of Kiaochow in 1898 and the operations of German "advisers" in China in the 1880s and 1930s particularly well. He has succeeded in producing an overview for the nonspecialist that is both useful and interesting.

WOODRUFF SMITH
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JOSEPH SMITH. *Illusions of Conflict: Anglo-American Diplomacy toward Latin America, 1865-1896*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1979. Pp. xv, 276. \$16.95.

This work on the Latin American diplomacy of the United States and Britain during the post-Civil War era is part of the current Latin American series of the University of Pittsburgh. The first two chapters are devoted to generalized discussions of British and American policy in the nineteenth century. Joseph Smith emphasizes Britain's peripheral interest in Latin America in contrast to America's drive for pre-eminence in the area, which did not disturb Britain unless its commercial interests were threatened. Thus, Britain (despite American suspicions to the contrary) was little more than an observer during the War of the Pacific and the other armed encounters of the period. It was increasingly content to stand aside and let the United States attempt to keep peace in the area.

The author next turns to the various isthmian canal projects. Here Britain's interest was occasionally stimulated by events, especially in 1882 when America charged that Britain had violated the Clayton-Bulwer Treaty on a number of occasions and sought to exclude Panama from the agreement. Quiet British diplomacy, as well as American political bickering, combined to permit Britain to maintain its rights intact at this time.

An interesting chapter traces the American drive to enlarge Latin American trade, culminating in the cleverly devised free list of the McKinley Tariff, which forced some Latin American states to make reciprocity treaties with the United States. Various factors caused the failure of the scheme, but not before America's increasingly intimate relations with Brazil had caused some consternation in London.

The writer then takes up American diplomacy in Argentina during the financial crisis caused by the collapse of Baring Brothers and the Chilean Revolution of 1891-92. Although America by no means won the friendship of these states, its political influence south of the border was obviously growing

apace. That influence, as well as the status of the Monroe Doctrine in international affairs, was confirmed by the Venezuela boundary dispute when Britain accepted an American demand to submit the question to arbitration. This incident, the author rightly observes, merely involved a public acknowledgment of a British policy long in gestation.

References to the private correspondence of the British statesmen of the period, which often reveal nuances of British diplomacy missing from the official documents subject to the scrutiny of the general public, would probably have strengthened the work in places. The author nevertheless effectively demonstrates the baselessness of most of the American suspicions and fears of Britain in Latin America, and his book provides a splendid introduction to a recent study by Warren G. Kneer, who takes up the Anglo-American story in 1901.

WILBUR DEVEREUX JONES
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WALTER ULLMANN. *The United States in Prague, 1945-1948*. (East European Monographs, number 36.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. x, 205. \$13.00.

The establishment of a new international political order in Europe in the 1940s will provoke debate among historians so long as they care to discuss the topic. That radical revision of power relationships has already proved to be relatively durable. Events of such magnitude do not promote the establishment of an easy orthodoxy of historical interpretation, and it is not only the grand view that is debatable. Particularist memories of the struggles in individual countries still arouse passions. Czechoslovakia is no exception. On the contrary, precisely because both sides in the contest hoped at the time that Czechoslovakia might prove different from the general pattern of events, the Czechoslovak case has remained especially controversial.

A new book based on newly available information must therefore be a welcome addition to historical literature. Walter Ullmann's study of American policy toward Czechoslovakia exploits the diplomatic archives in Washington, and that is its main value. The heavy reliance on this one source, however, is also the major weakness of the book, for the resulting narrowness of view sharply limits the potential impact of the monograph.

The material that Ullmann has gleaned from state department records contains no revelation radically different from the general views that have been aired previously. But it is intriguing to see policies spelled out in the words of the actors themselves, and the book contains candid glimpses of some of the major personalities in the struggle. The

central figure is American Ambassador Lawrence Steinhardt, whose reports supply most of Ullmann's information. Steinhardt's confidants such as Hubert Ripka, Jan Masaryk, and Eduard Beneš are the major figures on the Czech side. Steinhardt and his Czech associates alike were given to wishful thinking, as their persistent and mistaken optimism about the ability to contain Czechoslovak communism demonstrated. Given Steinhardt's reports, American policy makers in Washington must have been as surprised as were the democratic leaders in Czechoslovakia by the overwhelming Communist victories in the May 1946 elections.

Some historians have argued that the Czech Communists staged their coup in February 1948 because they saw that their electoral support was declining and that they would suffer losses in elections scheduled for the spring. It is worth noting that in his reports to Washington Steinhardt perceived no such decline and that the British Foreign Service, as well, could find little change in Communist strength. Ullmann also supplies other useful information on topics such as the debate within Czechoslovakia concerning the offer of Marshall Plan aid and the political struggles in Slovakia between the Slovak democrats and the Communists.

On the other hand, the major weakness of the book is that, despite his meticulous attention to detail, Ullmann's ultimate conclusions are frequently unsupported by hard analysis. Instead, his judgments seem to be influenced more by a desire to defend Czechoslovak policy and criticize American decisions wherever possible. He is impatient with the American lack of enthusiasm for the transfer of the Sudeten Germans. He thinks that the Americans were far too hard-nosed in their reaction to the Czechoslovak refusal to participate in the Marshall Plan. He disparages "Washington's political acumen" for allowing "such a prestigious position [as Bohemia] to fall into Soviet hands" (p. 137), yet three pages later he criticizes American policy makers for failing sufficiently to draw distinctions between Czechoslovak Communists and the Soviet Union. Ullmann criticizes American reaction to the February coup for failing to reassure the "Czechoslovak opposition about America's intention to restore democracy to their country" (p. 157). Just exactly how such a restoration could have been accomplished or what such an attempt would have meant for the international situation in 1948, Ullmann does not bother to explore. Instead, Ullmann relies simply and repeatedly on rhetorical phrases such as the need for "stronger medicine" (p. 154) and "a bolder defense of mutual hopes and aspirations" (p. 171).

The other main weakness of the book is the narrowness of its scope. Ullmann tends to treat American policy toward Czechoslovakia almost as if it were independent of all other considerations. For

example, in the first chapter Ullmann discusses the failure of the American army to occupy Prague in the spring of 1945 while barely mentioning the military situation in Germany and Austria or the broader military and strategic considerations that would logically have contributed to the decision. And the book is narrow in scope also in relation to the existing historiography of the outbreak of the Cold War. Certainly the Czechoslovak experience was a crucial part of the process by which the Grand Alliance broke into two competing alliance systems, yet little effort is made to draw connections between events in Czechoslovakia and the broader international political situation. In short, whereas the book does provide knowledge about American policy toward Czechoslovakia, the significance of that knowledge still requires explanation.

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University of Chicago

ANCIENT

R. F. WILLETTTS. *The Civilization of Ancient Crete*. Berkeley and Los Angeles: University of California Press. 1977. Pp. 279. \$22.75.

The restoration of Crete to its proper place in the history of Greece has been one of the central achievements of modern classical scholarship. R. F. Willetts has contributed significantly to this process in the past, and in this, his sixth book on ancient Crete, he has attempted to provide scholars and the general public with a comprehensive and up-to-date account of ancient Cretan civilization.

Willetts views Cretan civilization as the result of a continuous process of adapting foreign influences to local needs by the inhabitants of the island. Accordingly, three introductory chapters survey the geography of Crete, its relations with the other cultures of the eastern Mediterranean, and the contributions of the first settlers to the development of a distinctively Cretan civilization. The remaining thirteen chapters then treat that civilization in the bronze and iron ages. Relatively little space is devoted to the narrative of events. The focus instead is on the socioeconomic components of Cretan civilization and its cultural manifestations. Reflecting the differing natures of the source materials, archaeological for the bronze age and literary and epigraphical for the iron age, the section dealing with the former concentrates on the great Minoan palaces while that on the latter emphasizes social organization, law, and government. An appendix contains a translation of the Gortyn Code.

There is much to praise in *The Civilization of Ancient Crete*. Its author is thoroughly familiar with Crete itself, the ancient sources, and the voluminous

modern scholarly literature. His descriptions of archaeological sites are clear and his analyses of the relations between Cretan culture and its environment are often illuminating. Finally, every student of Greek history must be grateful to him for not only including a translation of the Gortyn Code in his book but also for making that difficult but important document accessible through a lucid analysis of its contents in chapters twelve and thirteen. Despite these virtues, however, there are serious methodological and theoretical flaws in the work that prevent it from being the sound *vademecum* to ancient Crete its author intended.

The first of these flaws is a surprisingly naive faith in the basic historicity of myth and the ability of archeology to validate that historicity. So we find, for example, the myth of the rape of Europa by Zeus cited as evidence for relations between Crete and Phoenicia and the Homeric characterization of Minos as *enneoros* as evidence for the theory that the Minoan Minos was a priestly figure who held his position for renewable eight-year terms. Such treatment of myth smacks of the nineteenth century, and, therefore, we are not surprised to find his analysis of the development of Cretan civilization dominated by the concepts of Victorian evolutionary thought. A variety of sources—the Messara tombs, neolithic figurines, the prominence of female figures in Minoan art and cult, and the lack of defenses and military themes in Minoan art—are adduced to produce the familiar picture of a peaceful matriarchal Minoan culture followed by a militaristic patriarchal Greek domination without any reference by the author to the virtual abandonment of the concept of matriarchy by modern anthropology or the revisions in the theory of a pacific Minoan culture that the military frescoes from Thera seem to require. These flaws seriously weaken Willetts's interpretation of Minoan civilization and limit his book's potential value to both scholars and the general public. A reliable and up-to-date survey of ancient Cretan civilization, therefore, unfortunately still remains a desideratum.

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ELEANOR GOLTZ HUZAR. *Mark Antony: A Biography*. Minneapolis: University of Minnesota Press. 1978. Pp. ix, 347. \$20.00.

This book provides a narrative account of the political events during Antony's lifetime. It does not offer a broad reinterpretation of his public career or new insights into the various scholarly conundrums posed by the period.

The footnotes contain few modern references, a lack that may explain evidence in the text that

Eleanor Goltz Huzar has not fully absorbed some of the major, recent contributions to our understanding of Roman politics. For example, she depicts the Senate as consisting of many "factiones" (in spite of Seager, *JRS*, 62 [1972]: 53–58); she refers to "political parties" and "party lines," using the anachronistic terms "liberal," "conservative," and "radical"; she believes the equestrians were the "commercial classes" (apparently unacquainted with C. Nicolet, *L'Ordre Equestre*) and, finally, confuses nobles with patricians when referring to the Gracchi as "patrician brothers who became tribunes" (p. 8).

There is a serious inconsistency in her attitude to the sources. For thirteen chapters she uses them uncritically as the basis of her narrative, but the fourteenth consists of a brief *quellenkritik* that suggests they are unreliable, products of a hostile Cicero and a propagandistic Augustan literary tradition, and therefore unlikely to give Antony a fair shake. This chapter is unfortunately bound to read as a disconcerting afterthought, for its conclusions have not in any way affected Huzar's approach to her sources in the main part of the book; instead of treating them as possibly tendentious or salacious propaganda, she assumes their accuracy. Accordingly, we read that Antony had "an inborn inclination for a carouse" (p. 24) so that his inability to resist a wild party not only damaged his reputation but also diminished his capacity to conduct crucial affairs the morning after; that early in his career he amassed a gambling debt to the tune of \$300,000 (*sic*) and purchased two beautiful serving boys for \$10,000 (*sic*); that he was politically inept, an incompetent domestic administrator, incapable of formulating long-range policies; and that he was a hopeless womanizer.

This is the picture before the *quellenkritik*. After it, Huzar tells us that Antony's political successes were considerable and must have depended on genuine practical virtues; that even his final failure was the product of virtue, of the moral inhibition that prevented him from being as "ruthless as the triumphant Augustus" and that he was a man who "relished being in love" (p. 254). The tone here belongs more to a whitewash than to an apologia. The fact remains that he authorized the grisly murder of Cicero and that he was, by Huzar's own admission, most responsible for the proscription of three hundred senators and two thousand equestrians. With Antony's blessing, every free man was promised one hundred thousand sesterces, every slave forty thousand and his freedom, when he presented the head of one of the twenty-three hundred proscribed to Antony or either of his colleagues, Octavian or Lepidus. Not even Huzar's explanation that Antony was simply "the product of a century of civil wars" (p. 254) can excuse that.

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RONALD SYME. *History in Ovid*. Oxford: Clarendon Press. 1978. Pp. 240. \$26.00.

Finding history in Ovid is a task to daunt lesser men. Poetic responsibilities are rich visions and excellent style, not sober, unexaggerated historical truth; and the Roman elegiac poets wrote of love and individualism with only incidental references to the world beyond their passions. But Ronald Syme draws on his redoubtable learning—and known convictions—to present Ovid as poet and citizen of Rome's empire in the later Augustan age.

Collecting all of the identifiable events and individuals, Syme compares Ovid's evidence with that of other poets, historians, and inscriptions and with prosopographical material to enlarge or correct our vision of Ovid's world. Triumphs, festivals, and construction of public buildings provide a close chronology for Ovid's life, poems, editions, even the sequence of poems within the *Tristia* and *Epistulae ex Ponte*. The evidence also argues for otherwise unattested matters of state, such as a German campaign and triumph for Tiberius in A.D. 13. Syme dares to trust Ovid even over Cassius Dio (lacunae in his text distort his record), Velleius Paterculus (devotion to Tiberius kept him silent about the successes of others), or Tacitus (his study of the Augustan age was superficial).

Individuals in the poems elicit Syme's formidable prosopographical skills. Ovid moved in many circles: his companionable *sodales*, poets like Propertius, "barbarian" friends of his exile. But when soliciting appeals to Augustus against his exile, Ovid named twenty-one leading *nobiles* in his *Epistulae ex Ponte*. Syme exploits these references to identify the individuals and determine who was in favor at a specific time during the reigns of Augustus and Tiberius. For Syme, who can affirm that "the central theme in the history of Rome is the enlargement through the ages of the senatorial order" (p. 95), Ovid's friends in the Senate, their places of origin, marriages, and political powers, are of central importance. Syme concludes that Ovid's ineffectual appeals even to friends of long standing show the opportunism and subservience of the Augustan *nobiles*.

The portrayal of Ovid's age (though only incidentally of Ovid) describes governmental patronage of and restraints on writers, as well as Augustus's moral and sumptuary legislation. Refuting other Ovidian scholars, Syme tries to clarify the vague charge of *carmen et error* (*Tristia*, 2.207) for which Ovid was relegated to Tomis. The improprieties of the *Ars Amatoria* were only public justification for the severity with which Augustus punished Ovid, who was unluckily present in a compromising situation that must have represented a political threat to Augustus. As in previous books,

Syme judges Augustus harsh and oppressive and rejoices that victory now rests with Augustus's victim, Ovid.

Regrettably, the book has deficiencies. Even thorough hunting and vigorous worrying of the few findings cannot compensate for the meager historical content in Ovid. Around the slight evidence Syme has drawn together chapters on the Augustan age that he acknowledges in the preface were written at different times and for different purposes. The integration is only partial. Moreover, Syme's Tacitean prose style remains cumbersome and distracting, an obstacle course to overcome. Notwithstanding, the book is valuable as quintessential Syme: profound erudition, fresh insights, and rich appreciation of the poet and the age.

ELEANOR G. HUZAR

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ROBERT BROWNING. *The Emperor Julian*. Reprint. Berkeley and Los Angeles: University of California Press. 1978. Pp. xii, 256. Paper \$4.95.

Late antiquity receives welcome scholarly attention in this superstitious age of ours, and Robert Browning is now having his say. This biography of the Emperor Julian is arranged in chronological, not topical, order. An introduction caricatures (as such chapters must, a colleague of mine insists) the age and its antecedents while a useful epilogue surveys the influence of Julian's life and personality on successive generations from his death until 1962, when Gore Vidal published his novel *Julian*. (Ironically Browning omits the strong fascination Julian had for the imagination of another backward-looking Hellene, C. P. Cavafy.) There are also a note on sources (a short list of materials in translation), suggestions for further reading (annotated for the general reader), Julian's family tree, table of dates, maps, and illustrations.

For Browning, Julian emerges as the protagonist of a Greek tragedy brooding over his identity and his condition, not always understanding his choices or their implications until too late. Shaping his account thus allows Browning to speculate about his subject's attitudes when the evidence is wanting, most noticeably during the youth's exile in Cappadocia. Occasionally, Browning reduces these silences to the trivial. When Julian, in exile, is haunted by the murder of his father and brother, he observes, "Whether these moments of intense emotion were worse than the long months of inescapable boredom only Julian himself could tell us, and he does not" (p. 43).

The story sometimes labors under its too linear narrative; issues must be discussed as the subject

meets them. The author's sense of Julian's development is thereby aided but not the reader's overview of relevant problems. Christianity, obviously important in the life of a man nicknamed the Apostate, gets fuzzy handling in this way. It is discussed throughout the book, and there is a major chapter on "Julian and the Christians" treating his attitudes and policies after accession. But there is a difficulty. In his introduction Browning surveys nearly three hundred years of Christian history and emphasizes its nonpolitical aspects. He overlooks theoretical considerations for persecuting the Christians on political grounds. To examine the Roman case, Browning should have explored the three ideas of kingship (divine, dynastic, Roman imperial) prevalent in the Palestine that invented Christianity. These were real issues for the Jews of the age and obviously affected the nascent Christian community. Taken with the political and military metaphors of early Christian writing, these ideas informed a tradition that was eventually abused to justify the atrocities of Christian emperors in the fourth century. Julian knew a great deal, as Browning insists, about Christian inconsistencies and Roman history; the reader should be allowed to explore this range also.

What he ignores is a strong intellectual tradition linking some attitudes of the early Christians, those of the Stoic opposition of the first century, the metaphors of the increasingly Stoicized Christian writers of the second and third centuries to fourth-century Christian and Cynic attitudes about defective monarchy or tyranny. This would have clarified how Christianity was "politicized" by Constantine (otherwise vague to the reader) and why contemporary Christians persecuted fellow Christians and pagans alike. (In general, the institutionalization of religious persecution as an ordinary device of political repression deserves more attention from the historians of this period.) My objection is that, although Browning deals with these subjects, the conflicts are not made precise in his treatment.

In short, Browning here shows the merits and defects of others of his recent books. He knows the sources well enough and generously states, as often as necessary, where the evidence fails us. We only wish that he had left us more markers to follow; footnotes would correct an impression of source mining. This volume meets the general reader's needs, not the scholar's.

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KEITH HOPKINS. *Conquerors and Slaves*. (Sociological Studies in Roman History, number 1.) New York:

Cambridge University Press. 1978. Pp. xiv, 268. \$22.50.

This stimulating and uneven book takes its title from the first of its five studies. The next two also concern slavery ("The Growth and Practice of Slavery in Roman Times" and "Between Slavery and Freedom: On Freeing Slaves at Delphi"), but the last two ("The Political Power of Eunuchs" and "Divine Emperors or the Symbolic Unity of the Roman Empire") relate to the others only in a concern for sociological features of their subject matter.

Keith Hopkins puts new questions to familiar material and seeks evidence for plausible answers. Every textbook of Roman history describes the growth of large estates and the importation of slave labor that marked the end of Italian small freehold agriculture in the later Republic. In his first study, subtitled "The Impact of Conquering an Empire on the Political Economy of Italy," he attributes this development to the continuous conscription of peasants for overseas conquest. Analyzing the complex economic factors and political issues involved, he shows why the importation of slaves relieved certain social conflicts and how Julius Caesar and Augustus brought a final resolution by the massive resettlement of Roman colonists in the provinces. The discussion is heavily indebted to Brunt's demographic studies, with many speculative assumptions, usually well marked, and with detailed examination of particular developments. An essay on "structural differentiation" in education, the army, and the law ends this section with a wide-ranging account of what "change" meant in the late Republic.

The second study focuses on the puzzle of widespread manumission. Why did an owner relinquish the wealth represented by a slave? Examining the conditions imposed on manumission, Hopkins concludes that in fact it brought economic advantages as well as personal and social rewards. The records of slave manumissions at Delphi provide the third study with a mass of precise data but without a social context: dated statistics of price, age, and sex are here more sharply defined, but Hopkins's insistent questions about motive and status and relationship can be answered only with a "possibly" or "perhaps." In contrast, the study of the power of eunuchs in the Eastern imperial bureaucracy is entirely one of context. An illuminating comparison with the position of the Court Jew in German states of the seventeenth and eighteenth centuries suggests one reason for influence that Hopkins hardly mentions: the absence of all hope of succession.

The infectious and occasionally ingenuous enthusiasm that enlivens the earlier studies emphasizes the inadequacy of the final one. That the Romans took emperor cult seriously, that it had sociological consequences, that there were parallels to Christian

attitudes and language, and that the supernatural loomed large in Greco-Roman life—these have been matters of intense research. The chapter in the *Cambridge Ancient History*, volume 10, on religious developments covered this same ground years ago with fuller and more sensitive discussion even of the sociological repercussions. Both the approach and the bibliographical knowledge in this final study are narrow and out of date. It mars a spirited and informative book.

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MEDIEVAL

NORMAN DANIEL. *The Arabs and Medieval Europe*. (Arab Background Series.) 2d ed. New York: Longman, and Librairie du Liban, Beirut. 1979. Pp. xiv, 395, £9.50.

This is the second edition of an important book that examines the relations between the Arabs and Latin Europe during the Middle Ages. Its author, who has written extensively on this general subject for the last few years, here sums up his views as to how the Latin West approached and dealt with the Muslim world during some six medieval centuries. Norman Daniel begins with a general sketch of the state of the Islamic and Latin worlds in the eighth century. Then he considers a number of topics, beginning with ninth-century Spain at the time of the "Martyrs of Cordova" and continuing on with an examination of the tenth century in the western and central Mediterranean, the origins of the Crusades in the late eleventh century, Italy and the Kingdom of Two Sicilies from the eleventh to the thirteenth century, and the crusading states of Syria and Palestine down to 1291. His last three chapters are concerned with Christian religious conflicts with Islam, the reception of Arab science in the West, and the last years of the Middle Ages, which saw the deepening of the conflict between these two civilizations.

The author's approach is to examine all available contemporary Latin sources, concentrate upon them, and confine himself to areas like Spain, Sicily, Syria, and Palestine, where the drama of confrontation was played out during these centuries. He is to be congratulated upon the depth of his learning and the completeness of his coverage of relevant Latin Western source material. What emerges from his study is a view of the Latin West that emphasizes its unwillingness and inability to come to terms in any meaningful way with either Islamic civilization or Muslims during these medieval centuries—on whatever level one chooses to examine the confrontation or in whatever part of the Mediterranean one wishes to consider. He is espe-

cially critical of the way in which rulers of Sicily like Frederick II approached the Islamic world, and he doubts that Christian Spain was much more enlightened in coming to terms with its Muslim brethren and their culture—despite much that has been written to the contrary.

Valuable as all of this is, one cannot but be struck by the one-sidedness of the evidence Daniel presents to us, for despite his all-inclusive title he gives us only Latin European views and actions and never those of their Islamic counterparts. Save for a few twelfth-century Syrian Arab writers, we are never told of how historians like Ibn Al-Athir, Nuwairi, Juvaini, or Raschid-al-Din viewed the Franks; Ibn Khaldun's famous remarks on Christianity are not even mentioned. When we get to the later Middle Ages nothing is said of Mamluk historians like Ibn Taghri Birdi, who had much to say concerning Latin Westerners.

It is possible, of course, that if one included the views of such historians, one would not change the overall picture that the author has given us, but how can one be sure? Confrontation between civilizations is a two-way affair, and, excellent as Daniel's presentation of the Latin West is, the other side needs to be weighed or his title should be changed to *Medieval Latin Europe's View of and Dealings with the Arabs*.

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LESTER K. LITTLE. *Religious Poverty and the Profit Economy in Medieval Europe*. Ithaca, N.Y.: Cornell University Press. 1978. Pp. xi, 267. \$27.50.

Lester K. Little's "broad, interpretative essay" is an account of the role played by voluntary poverty in the new profit economy of Western Europe between the eleventh and the fourteenth centuries. His particular interest is in the ways by which ideas of voluntary poverty helped shape a kind of spiritual life that accommodated the merchants, town dwellers, and clerics whom the new economy brought into prominence. The book is a synthesis of much of the most interesting and important recent medieval scholarship on theories of society, economics, morality, and lay and clerical spirituality, a good deal of which is by Little himself.

Little begins (part one, "The Spiritual Crisis of Medieval Urban Culture") by considering the cultural consequences of the growth of a profit economy in terms of Georges Duby's model of the shift from a gift economy to a profit economy. The immense changes and challenges to the aristocratic-monastic culture of early medieval Europe that even the small-scale urbanism and commerce of the

twelfth century caused shaped new attitudes toward wealth, money, and poverty and lay behind the deterioration of Jewish-Christian relations that so indelibly marks the late eleventh and twelfth centuries. "The guardians of conscience," however, monastic representatives of the aristocracy whose spirituality and view of the lay world was closely tied to aristocratic values, "were not prepared with an ideology or an ethic favorable to urban life" (p. 35). To them, the city was still the legacy of Cain and the home of avarice, a term that, as Little shows, became a blanket condemnation of the shift from a rural gift-aristocratic culture to an urban exchange-profit-commercial-civic culture, particularly in northern Italy, France, and the lower Rhine area in the late twelfth and thirteenth centuries. The tone of these condemnations seems anticipatory of some of the criticisms of early nineteenth-century England in the work of William Cobbett and Sharon Turner.

Some representatives of the older cultural values (part two, "Avoiding the Crisis: Monks and Hermits") made what Janet Nelson has elsewhere called "a heavy investment of religious capital" in old forms—traditional monasticism, purged and once again withdrawn from the world, as at Chartreux and Citeaux. It is useful to compare Little's treatment of this topic with that of Alexander Murray (*Reason and Society in the Middle Ages* [1978], pp. 314–404). Others, however, (part three, "Confronting the Crisis: Canons, Laymen, and Friars") faced the cultural implications of the twelfth century head-on. Little's remarks here, particularly on dissidents, heretics, and the mendicant orders, illustrate the process by which voluntary poverty in the face of rising wealth became the chief element in that confrontation. His focus on spirituality builds solidly on the earlier work of Herbert Grundmann and M.-D. Chenu and avoids the usual pitfalls of discussing spiritual reform in materialistic terms.

Part Four ("The Formation of an Urban Spirituality") emphasizes the role played by scholastic social thought, developed independently of the religious movements Little has considered so far, but later made their own by the Franciscans and Dominicans in their "reformed apostolate" to the towns. The chief elements of this reformed apostolate were preaching, a new emphasis upon the psychological dimensions of penitence, popularizations of the new learning in handbooks for confessors and preachers, and the orders' support of lay piety. The mendicant orders, with their response to the urban demand for talk and persuasion, their spiritual guidance for people in diversified and particular walks of life, their recognition of the urban sense of fraternity, and their cults of urban saints, combined to create an appropriate spirituality for the new life in the towns and the new lay intellectual and mate-

rial culture. Little's book describes how one dimension of thirteenth-century civic culture—the creation of an adequate and appropriate spirituality for townspeople—came to be the great spiritual and social achievement of the mendicant orders.

There are a number of editorial and typographical errors, and even the pared-down bibliography in the notes suffers from the absence of such names as Robert Bultot and John Mundy. But these are minor objections. Somewhat more serious is the argument that Little should have had more space and leisure to develop the argument more fully. As it stands, the book may put off some readers a little by the sharpness and categorical character of many of its statements. But this quality is the result of the book's brevity, not its scholarship or intellectual rigor. This work should speak to more historians than medievalists alone and should take a distinguished place beside the earlier studies on similar subjects by Benjamin Nelson, Raymond de Roover, John Baldwin, Jacques LeGoff, Pierre Michaud-Quantin, Duby, and Chenu.

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JAROSLAV PELIKAN. *The Christian Tradition: A History of the Development of Doctrine*. Volume 3, *The Growth of Medieval Theology (600–1300)*. Chicago: University of Chicago Press. 1978. Pp. xxvii, 333. \$17.50.

In this third volume of his five-volume history of Christian doctrine Jaroslav Pelikan provides students of medieval intellectual history and the history of dogma with an unusually valuable supplement to and reworking of the classics in this field. Many of these older works traced the evolution of medieval Christian doctrine through a series of major theological controversies and the ideas of key ecclesiastical figures. Pelikan takes an alternate approach by examining major doctrinal *loci* found in the works of both the stars and lesser luminaries in the medieval theological galaxy. What is especially impressive about this synthesis is that it is based not so much on secondary literature (which the author clearly knows) but on a fresh and perceptive reading of a vast number of primary sources ranging from the standard theological treatises to liturgical commentaries, canon law, and vernacular literature.

The volume is arranged chronologically, but each chapter, given a thematic title, may describe doctrinal evolution across several centuries, because theological topics are treated in the century in which they reached their consummate stage of development. Thus, medieval doctrines of redemption are clustered in the chapter discussing Anselm and Abelard.

Chapter one, on the seventh and eighth centuries, examines the early medieval elaboration of the consensus of faith received from patristic antiquity, especially from the credal statements and Augustine. The doctrinally rich ninth century, treated in the second chapter, is considered as a period of reworking the Augustinian synthesis; not only are the well-known controversies over adoptionism, predestination, and the eucharist described but also trinitarian doctrine and the beginnings of Mariology. The "christocentric period of Western Civilization" from 900–1100 provides the focus for the third chapter, which examines medieval atonement theory on the basis of its classic statements in Anselm, Abelard, and Bernard together with an unexpected mélange of sources ranging from Odo of Cluny to the *Dream of the Rood*, *Victimae paschali*, and Cynewulf's *Christ*. Chapter four, the most brilliant of the volume, deals with the way in which medieval theologians and liturgists described the multiple channels of grace in Mary, the saints, the eucharist, and other sacraments. During the twelfth century, treated in chapter five, the problem of authority became a crucial concern in Christian doctrine and was manifest in a multiplicity of issues—the relative values given to Scripture and the Fathers, the Church's response to popular and doctrinal heresies, the encounters of the Christian faith with Muslim and Jewish systems, and the relationship between faith and reason. The final chapter of the volume describes the thirteenth century as a period not so much as one of growth of Christian doctrine but as one of consolidation, integration, and summarization of the Catholic tradition. Aspects of the doctrines of predestination, Trinity, and the *filioque* were reopened and examined with the new tools of scholastic method, and there was a systematization of aspects of angelology, ecclesiology, and eschatology.

To criticize this volume for its lacunae and slighting of various topics will clearly be a temptation for any reviewer. There will undoubtedly be grumblings that this or that specialization of the reviewer has not been accorded its rightful amount of space or has been misplaced in another volume. One thinks, for example, of the paucity of "liturgical theology" drawn from the liturgical formulas themselves or of a periodization of medieval doctrine that ends with the thirteenth century. But, whatever the lacunae, they are more than amply compensated for by the scope of the book and by its treatment of aspects of medieval doctrine rarely touched upon in the standard handbooks. Among these might be mentioned the origins of medieval Mariology in the ninth century, the "leap theory" of redemption, the place of the saints in Christian prayer, and Muslims and Jews in medieval Christian doctrine. All of these forays into topics rare in

general medieval intellectual histories and histories of doctrine would have been impossible without an intimate acquaintance with a great diversity of theological sources and the ability to see connections where few have been noticed before. Pelikan's volume—clearly to be numbered among the classic surveys of medieval doctrine—is an invitation to students to read anew the primary sources of medieval theology and to be sensitive to the subtle and related patterns in the intricate web of medieval doctrine.

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PAUL SPECK. *Kaiser Konstantin VI. Die Legitimation einer fremden und der Versuch einer eigenen Herrschaft: Quellenkritische Darstellung von 25 Jahren byzantinischer Geschichte nach dem ersten Ikonoklasmus*. Volume 1, *Untersuchung*; volume 2, *Anmerkungen und Register*. Munich: Wilhelm Fink Verlag. 1978. Pp. 419, 424–857. DM 48; DM 48.

The reader's initial reaction to this work is to ask how it is possible for anyone to write more than eight hundred pages about the very poorly documented reign of Constantine VI. Paul Speck, however, succeeds in accomplishing what he has set out to do, that is, he provides the most thorough study of the little-studied Constantine VI that has ever been attempted. In fact, very little has ever been written on Constantine VI, and there is no other work on his reign. Speck's study should remain standard for a long time even though he cannot give definitive conclusions to all of the historical questions he discusses. This work, Speck's third, takes a historical approach, while his earlier books were really works of textual edition, criticism, and literary history. He has discovered no important new sources but has attentively and critically read the existing ones, except for some untranslated Arabic materials, and he knows the modern scholarship quite well.

The first volume contains the text and the second appendixes and copious footnotes that discuss and explore many technical questions. Speck analyzes the entire late eighth-century reign, taking up problems in chronological order and concentrating on political, military, and religious history. The work's greatest value lies in his probing analysis of the internal history of the reign. He offers many hypotheses, not all of which can be proven; but his hypotheses are reasonable, even if they will not satisfy everyone. He has made a fundamental contribution to the understanding of iconoclasm, and he correctly concludes that it is impossible and indeed incorrect to attribute any monolithic social basis to it.

He provides almost no discussion of institutional, economic, or cultural topics, but the sources do not permit discussion of them. The work is dry reading, a serious and sober piece of research that probably will not have a wide audience. Medievalists will find that Speck does make useful observations about Carolingian and papal relations with Byzantium. But his discussion of Byzantine-Arab relations suffers from a lack of knowledge of the Arabic materials, including the recently published history of Ibn A'tham.

In general, Speck demonstrates a judicious weighing of the extant sparse evidence on many topics of internal history. His study will be an indispensable tool for specialists because of his thorough familiarity with the sources and his long contemplation of many difficult questions of the late eighth century. The appendixes and indexes are detailed. A *Habilitationsschrift*, it is another noteworthy accomplishment of Byzantine studies at the University of Munich.

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SIGFUS BLÖNDAL. *The Varangians of Byzantium: An Aspect of Byzantine Military History*. Translated, revised, and rewritten by BENEDIKT S. BENEDIKZ. New York: Cambridge University Press. 1978. Pp. xiii, 242. \$35.00.

The strange story of the Varangian corps—the body of Viking mercenaries who served as the life guard of the Byzantine emperors—has always had a special attraction for the English-speaking and Scandinavian worlds. Vikings from Sweden followed the Russian waterways that they ruled south to the imperial capital on the Bosphorus, where their legendary military prowess gave them easy entree into the Byzantine army. First they came as individual recruits to be absorbed into various regiments. But, from the last years of the tenth century on, they normally came to be part of special Varangian military corps, most notably the personal guard and crack troop of the Byzantine emperor himself; they were the famed and dreaded “ax-wielding barbarians.” The Varangian regiments were particularly important in the eleventh and twelfth centuries when, along with other mercenaries, they came to replace the dwindling thematic national army of the Byzantine state. The high point of Scandinavian activity in Byzantium coincided with the career of Harald Haardraade, half-brother of St. Olaf the King, son-in-law of the grand prince of Kiev, and himself king of Norway. Having fought as a junior officer with the Byzantine army in Sicily, Asia Minor, and Bulgaria, he was destined to die at Stamford Bridge in 1066.

Surely this is the stuff of novels as much as of history!

Benedict S. Benedikz has certainly done historians a service by translating and updating Sigfús Blöndal's basic work on the Varangians in Byzantium. Almost without exception, the original author and his reviser record and analyze every shred of evidence that touches on the Vikings on the *Oesterveg* or in the empire, or that might conceivably refer to Norsemen and their activities at the eastern end of the Mediterranean. The authors have scoured the voluminous Norse saga-related literature and reproduced here most of its references to the Byzantine Empire; they have weighed and evaluated these references and attempted to relate them to each other and, when possible, to historical sources from elsewhere. Anyone seriously interested in Varangians in Byzantium must consult this book.

The book, however, is in many ways unsatisfactory. The authors' conscientious exegesis of any material even potentially relevant to questions of Viking service in Byzantium leads constantly to chains of interesting, but unconvincing, conjectures dependent one upon the other and adding nothing to real knowledge about the subject. The introductory chapter on the Viking penetration of Russia rehearses in minute detail material long since available elsewhere. The footnoting is careless and undependable, and the book is not particularly well written. Finally, a consistent picture of the Norse and later English “Varangians” in Byzantium never emerges from the mass of accumulated facts and surmises. Although this volume will sometimes prove extremely suggestive on specific questions, for a more balanced tracing of the role of the Varangians in the Eastern Roman Empire, treated, indeed, in a wider historical perspective, one might better consult Hilda Roderick Davidson's recent *Viking Road to Byzantium* (1976).

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CHARLES R. YOUNG. *The Royal Forests of Medieval England*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1979. Pp. ix, 221. \$14.00.

This is a highly readable general history of the English royal forest system from the Normans' introduction of the technical concept of royal forest until the later Middle Ages, when this system declined. The treatment is fundamentally chronological, the preface and introduction being followed by chapters on the Norman kings' organization, the Angevins' reorganization, and the Angevin system at work. The fourth chapter shows the forests be-

coming a political issue around the beginning of the thirteenth century, when the king's exploitation of them made forest administration, which already had legal, economic, and social repercussions, an important baronial grievance. After two chapters on the law and administration of the forests and on their economy when the system was at its height in the thirteenth century, attention is refocused on the political aspect of the forests in the later thirteenth and early fourteenth centuries. The final chapter charts the decline of the forests as an institution after 1327. There are helpful footnotes, a selective bibliography, and an index; three maps, five tables, and two graphs buttress the text.

Charles R. Young seeks "the overall pattern of forest administration buried in the detail of record sources" (p. viii) and disregards individual variation in order to establish main trends. His work highlights some interesting aspects of English medieval history, for the royal forests, previously studied mainly individually, were not merely regional phenomena or simply wooded land. In the thirteenth century a quarter of the land was technically forest and the identifiable royal forests were spread widely over England. In addition to the common law, a particular forest law, with its own officials and courts, applied within the area designated as royal forest. Young describes the evolution, maturity, and decay of the forest system, clearly sets out the hierarchy of officialdom and courts, and probes the relationship between forest administration and the ordinary local and central administrative systems.

The economic exploitation of forest resources can be traced through government records, and Young's assessment of the economic value of the forests in relation to contemporary resources and needs helps to explain the forests' political significance. The royal forests produced for kings goods in kind, cash profits, and opportunities to exercise patronage; but landholders resented controls on estates held within designated forests, and forest laws were seen as arbitrarily and capriciously enforced. After 1327 the forests' extent was limited and the particular forest administration began to disintegrate. The royal forests, becoming economically insignificant, declined into mere hunting preserves much as they had begun.

The complicated technicalities of forest administration are clearly explained in this comprehensive book, and Young's humane interest in his sources leads to the inclusion of many illustrative cases that enrich his text.

HELEN M. JEWELL
University of Liverpool

JOHN J. CONTRENI. *The Cathedral School of Laon from 850 to 930: Its Manuscripts and Masters*. (Münchener Bei-

träge zur Mediävistik und Renaissance-Forschung, number 29.) Munich: Arbo-Gesellschaft. 1978. Pp. xv, 212. DM 25.

In the third quarter of the ninth century, Laon formed an important link in a chain of cultural centers responsible for the Carolingian renaissance, inasmuch as its cathedral school transmitted to an entire generation of Continental scholars the fruits of the intellectual labor of a small group of Irish scholars, most notably Martin Hiberniensis and John Scottus Eriugena. Despite this, little has been known about the school's masters, students, and books or about its nature and influence, largely because of the paucity of sources available for the history of the school.

John J. Contreni set out some years ago to remedy this deficiency, producing an intellectual history of the school as his doctoral dissertation. Since then he has made a fine translation of Pierre Riché's *Éducation et culture dans l'Occident barbare* (1976) and has learned much from Riché's technique of going beyond narrative sources to milk the last drop of significance out of diplomatic, juridical, epigraphical, and archeological sources. This has led him in the present work to retain from his dissertation only the first three chapters, and in the remaining nine to shift from intellectual to institutional history, tapping a previously unexploited source, the school's manuscripts, to supply important detail regarding the composition of the library and the operation of the scriptorium at Laon's cathedral school. His book begins with a detailed analysis of the ecclesiastical and political history of ninth-century Laon, which does a great deal to clarify the nature and extent of the indispensable support given the school by the city's bishops. It then examines the manuscripts in detail, illuminating the formation and composition of the library as well as the origins of its manuscripts. In the final section, it discusses the teaching activity of the masters, calling attention to the central role of Martin Hiberniensis and drawing conclusions about the school's students and influence. The book is thorough in documentation and provides appendixes detailing the contents of the library and presenting a concordance of shelfmarks of ninth- and tenth-century manuscripts. A bibliography, general index, and index of manuscripts are included.

The greatest drawback of this book, one that Contreni himself points out, is that it is the study of a single school in isolation, without reference to its complex linkages to other centers of study. The fault is not the author's. There is need of a study of all ninth-century intellectual centers, and Contreni suggests that it ought to be, like the present work, an archeological investigation of culture, performed codicologically by reconstructing libraries, deter-

mining the origin and provenance of manuscripts, and studying their notes and text traditions. Contreni's provocative and sound study is a worthy milestone along the road to such an investigation.

JOSEPH M. MCCARTHY
Suffolk University

MALCOLM BARBER. *The Trial of the Templars*. New York: Cambridge University Press. 1978. Pp. vii, 311. \$28.50.

The intent of the author of this book was to make available, in English, a complete account of the trial of the Knights Templar and to demonstrate the relevance of that event to the political turmoil of the twentieth century. In the first of these goals he succeeds admirably; in the second he does not. Since the first purpose is by far the more important, the book can still be judged a significant contribution to the literature on the Templars.

Increasingly, students of our educational system are unable to read foreign languages. In their study of medieval history this means simply that a large body of scholarship on many important topics is inaccessible to them. Translation alone is not the answer; there is little to be gained by translating old interpretations. Malcolm Barber's study is the only proper response to current academic needs. It is a judicious blend of old and new work, past and personal interpretation, written in a good descriptive English prose style. The modern student now has available a thorough and balanced account, incorporating all prior studies, of one of the more interesting and important episodes of the Middle Ages.

Barber's presentation of the facts of the proceedings against members of the crusading order is a very straightforward descriptive account, almost a day-by-day record of a six-year event. Enormously detailed and heavily documented from original sources, these chapters convey by sheer weight of information the complexity of Church-state relations in medieval society and the conflicting motivations of all of the principal characters in the great drama of the downfall of an order that medieval men had once held in awe. Particularly valuable in regard to evidence are the appendixes where the reader can find the original articles of accusation brought against the order in 1308 and the description of a reception into the order in 1311. One cannot quibble, let alone quarrel, with the author's mastery of the sources for his subject.

The conclusions that Barber draws from his intensive study of the trial proceedings and of the political maneuverings of popes and kings during the process are well balanced and well argued but not highly original. Serious students of the suppression generally agree that the Templars were unjustly ac-

cused and convicted, that Philip IV was greedy for their wealth, that other European monarchs acted better than he, and that Clement V was a weak pope. The reader seeking a new interpretation will not, therefore, find what he is looking for in Barber's conclusions. Personally, I do not regard this as a failing. We have for too long in historical writing suffered from "extravagant originality." Barber's conclusions are fully supported by his facts and that is sufficient for any scholarly study.

The author's belief, stated in the preface, that the trial of the Templars "has some relevance to the world of the late twentieth century" is curiously irrelevant. He never attempts to demonstrate this and the evidence does not support it. It is difficult to know what he had in mind. In any event, this unfulfilled promise is inconsequential considering the ample merits of what he has done.

C. LEON TIPTON
Lehigh University

JEAN FAVIER. *Philippe le Bel*. Paris: Fayard. 1978. Pp. v, 587.

"He is neither a man nor a beast. He is a statue." Such was the assessment of Philip the Fair by Bernard Saisset, the notorious bishop of Pamiers. Unlike such figures as Charlemagne, Henry II of England, Saint Louis, and Frederick II of Sicily, whose personalities and character emerge however dimly from the available medieval sources, Philip the Fair has remained particularly enigmatic. Historians have been virtually unable to piece together from narrative and nonnarrative sources any of the personality of this last great Capetian, an especially frustrating situation because his reign is of special significance for the development of French institutions, the transformation of the Church, and the secularization of society.

What was Philip's role in the many devaluations of the French currency and the inflation, French expansion in the Midi, the traumatic conflict with Flanders, the epic struggle with Boniface VIII, the destruction of the Templars, and the summoning of the assemblies? Was he a champion of nationalism and a precursor of secularism? Was he capable of true friendship and loyalty? Did he initiate, implement, and control policy? Historians have wrestled with such questions since Pierre Dupuy first examined the records of Philip in the seventeenth century. Romantics like Dumas and Michelet blamed Philip for the notorious crimes and dramatic incidents of his reign, whereas positivists like Charles-Victor Langlois and Robert Fawtier ultimately concluded that it was impossible to assess Philip's influence. A contemporary historian who believes it possible is Joseph R. Strayer. In a most convincing

article he has argued that Philip, rather than such councillors as Nogaret, Flote, and Marigny, was responsible for what was done.

Mindful of the difficulties of studying the reign of Philip the Fair, Jean Favier, director of the Archives de France, has virtually marinated himself in the sources and has written many studies on various aspects of Philip's reign. No one is better equipped to write about Philip the Fair, and in this book Favier has done a remarkable job, paying special attention to the king, his council, the household, the royal servants, monetary policies and finance, war, diplomacy, the struggle with Boniface VIII, and the affair of the Templars. He portrays Philip as no great innovator but as one who honored the tradition of Saint Louis and operated within a feudal ambience and mentality. Nothing Philip did can be interpreted as a dramatic rupture with the past and a leap into the modern world.

To see behind the mask of Philip is not easy because, as Favier so well observes, Philip listened more than he spoke and became involved in a decision or a negotiation only at the opportune moment. Although some of his councillors and officials specialized in certain tasks and business, none set or dominated policy; Philip was always in control. In disagreement with attempts of other historians to portray some of Philip's principal councillors as Machiavellian, Favier sees them as but reflecting the political ideas of a Thomas Aquinas. His treatment of Philip's conflicts with Flanders, Boniface VIII, and the Templars is eminently fair. He concludes that Philip did not covet the material resources of the Templars but wanted to remove from his realm this crusading order that had become anachronistic and remained a kind of independent enclave responsible only to the papacy. If Philip was ruthless in the battle with Boniface VIII, perhaps it was because Boniface was unrealistic in the authority he thought he could wield over early fourteenth-century Europe. Regarding the feudal lordship of the Capetians over Flanders, Favier does not question the legality but the manner of Philip's intervention in Flemish affairs.

Throughout the text Favier quotes extensively from key sources but generally develops an argument or comes to a conclusion without citing the specific evidence, which the reader must infer from the sections devoted to sources and an *orientation bibliographique*. This is unfortunate and unfair to those scholars, particularly American, whose recent studies have advanced our knowledge of the reign of Philip the Fair, some of whom are not even cited in the *orientation bibliographique*. One is often left uncertain and confused. Had Favier used footnotes, Philip might not have remained the enigmatic statue that he still does. Yet this book, developed from an extraordinary knowledge of the primary

sources and secondary materials, can only elicit admiration. Its sensible and moderate course generally places the well-known events and policies within the broad context of European and French history. It will now be doubly interesting to read the study of Philip the Fair by Strayer that reportedly is soon to appear.

BRYCE LYON
Brown University

THOMAS N. BISSON. *Conservation of Coinage: Monetary Exploitation and Its Restraint in France, Catalonia, and Aragon (c. A.D. 1000-c. 1225)*. Oxford: Clarendon Press. 1979. Pp. xxiii, 250. \$33.00.

The coinage of medieval Western Europe is an important subject, of interest not only to economic and art historians but also to scholars studying representative assemblies, political debate, and legal theory. In an economy of expanding trade and fixed rural rents, the coinage was a matter of great public concern, yet the lords who operated mints regarded them as private and potentially profitable enterprises. Good historical studies of medieval coinage have been hard to come by because historians and numismatists have been reluctant to become deeply involved in each other's specialties. These considerations make Thomas N. Bisson's book particularly welcome. It deals with a complicated subject, and much of it makes difficult reading; but it is a work of extraordinary scholarship. It throws important light on medieval attitudes about money and on economic and political problems that have been difficult to study because of innumerable regional differences that tend to defy generalized treatment.

Historians are more familiar with the politics of coinage at a later date, notably the fourteenth century, when rulers of large territories manipulated the currency in order to attract bullion and to make a profit from the mints. The profits amounted to a disguised tax on the rich and created a political uproar that has been easier to study and to generalize about because the debates were conducted at a national level. Bisson has tackled the more difficult task of examining the earlier, regional debates over the coinage. He has started at the beginning, the eleventh century, when coinage followed the path of other once-public functions and became essentially proprietary. As this happened, "the disappearance of the uniform Carolingian denier and the progress of commercial exchange combined to foster a heightened sensitivity to monetary value" (p. 12). The first reaction to this development was a demand to stabilize the coinage, not merely the weight and alloy (which seem to have received less emphasis than one would expect) but also the ac-

tual design of the coins. In some regions, rulers who agreed to mint no new coins were not even to replace worn issues with new coins of identical characteristics. This "conservation of the coinage was easily among the more reactionary economic notions known to history, [a] child of rural anxieties. . ." (p. 196).

Bisson considers in turn Normandy and the old Capetian domain, Languedoc and Catalonia, and the regions of northeastern France. Rulers confirmed or guaranteed their coinages under a variety of different circumstances and political pressures. In Normandy and the Capetian lands, inhabitants paid a small regular tax to guarantee a stable coinage. In Catalonia, by contrast, they paid less regular, but larger, amounts to secure confirmations of coinages by rulers whose mints apparently were profitable. After the 1180s, economic realities everywhere began to moderate the old desire to maintain the same currency. Popular concern shifted toward a desire to maintain some control over mutations and rates of exchange.

These few words cannot do justice to this excellent book. It contains an impressive bibliography, an appendix of important documents, and facsimiles of both documents and coins that attest to the author's scholarship.

JOHN BELL HENNEMAN
University of Iowa

ROGER BOASE. *The Troubadour Revival: A Study of Social Change and Traditionalism in Late Medieval Spain*. Boston: Routledge and Kegan Paul. 1978. Pp. xvi, 219. \$23.95.

In *The Troubadour Revival* Roger Boase asserts that troubadour poetry transcended the bounds of literature and that courtly love "was a literary and sentimental ideology, a secular profession of faith, which . . . was the chief impulse behind the cultural achievements and the style of life of the European aristocracy" from the twelfth to the sixteenth century (p. xi). Spain saw a substantial revival of troubadour literature and chivalric attitudes during the late fourteenth and fifteenth centuries, and Boase believes that this was due to Spain's unusual historical development, particularly the growth in numbers and influence of the aristocracy at a time when the military justification for its status was waning. As a consequence, Spain's high nobles "retreated into an anachronistic world of make-believe, and cultivated literary conventions and chivalric practices associated with a utopian past" (p. 5).

This, on the surface, is a plausible thesis. To prove it, Boase first provides a static description of the aristocratic theory that divided society into the familiar three estates—*defensores*, *oratores*, and *labora-*

tores. He demonstrates that many Spanish writers accepted and supported this division, even though it did not take into account the developments and differentiations in the third estate. In the same section, he shows how patronage regulated relations between the estates. Next, he examines Spanish history with the special aim of showing the reasons for the growth in the numbers of aristocrats in the fifteenth century. Finally, he prints a selection of documents to show that Aragonese, Navarrese, and Castilian rulers encouraged chivalric practices and sponsored academies of the "gay science" of chivalric behavior.

But Boase's evidence does not sustain his thesis. It is hardly surprising that fourteenth- and fifteenth-century Spanish writers maintained a belief in the three-fold division of society; it was widely held in Europe long after the fifteenth century. More important, Boase's survey of Spanish history is flawed by his reliance on an outmoded view of it. This is most apparent in his attempt to explain the rise of the aristocracy mainly by focusing on the role of the kings, who, facing external wars and internal contentions, created new titles and promoted new nobles. This is partially true, but what of the nobles themselves? Boase gives the impression that the sole utility of nobles was war and that, once their military importance declined, they no longer had any reason to exist. A wider reading in Spanish history, particularly of those historians who have analyzed the nobility, could have shown him a great deal more about noble attitudes, about the history of noble families, and about the economic activities of the nobles, many of whom were vitally interested in trade. The documents he reproduces in the third section could be used to support a modified thesis: that the monarchs, interested in building royal power, deliberately fostered an archaic ideology to bring the nobles under control.

Boase's scholarship is wide-ranging, and some of the points he makes are sound and useful. It is unfortunate that his overall analysis is ultimately unsatisfying.

WILLIAM D. PHILLIPS, JR.
San Diego State University

J. N. HILLGARTH. *The Spanish Kingdoms, 1250-1516*. Volume 2, *1410-1516, Castilian Hegemony*. Oxford: Clarendon Press. 1978. Pp. xi, 716. \$39.95.

This continuation of J. N. Hillgarth's study of Spain in the later Middle Ages extends from the Compromise of Caspe to the death of Ferdinand of Aragon. The first part considers economic, social, religious, and cultural matters, while part two deals with political history to the death of Enrique IV of Castile. Part three, constituting almost half the

book, is devoted to the reigns of Ferdinand and Isabella.

One can comment on only a few of the many topics discussed. Hillgarth rejects the notion that the decline of Catalonia was the consequence of a series of events going back to the Black Death and argues that it can be demonstrated only after the civil war of the late fifteenth century. The most important economic development was the rise of Castile and Portugal whose opening to the Atlantic gave them an advantage in the early modern era that Catalonia lacked. The collapse of the centuries-long coexistence of Christians, Muslims, and Jews was of exceptional significance. Although the Moors seem not to have attracted intense hostility, anti-Jewish sentiment grew ever stronger and was encouraged by Jewish converts to Christianity. The push to suppress religious groups other than Christians was primarily a popular movement that did not win the support of the higher powers until the end of the fifteenth century. Also noteworthy is the limited influence of humanism in the peninsula. The first Catalan humanists were men attached to the royal court such as Bernat Metge, but in Castile they were nobles such as Santillana or *conversos* such as Cartagena. Traditional cultural strains, however, remained paramount.

In political terms the fifteenth century was a time of Castilian ascendancy, beginning with the establishment of the Trastamaran dynasty in Aragon following the Compromise of Caspe. This had the effect of creating a breach with the dominant classes in Catalonia and led to the civil war, so destructive of Catalonia's position in the Crown of Aragon and in the peninsula. The marriage of Ferdinand and Isabella effected a personal union of Aragon and Castile, but rather than see these two rulers as conscious creators of a modern state, Hillgarth thinks that they built upon and continued the traditions of the past, especially those of Castile. Their principal innovation was to make existing organs of government work more effectively than ever before. They departed from tradition in two ways. First, their marital diplomacy brought about the accession of Charles V and a foreign dynasty that involved Spain in northern European politics. Second, their determination to achieve religious unity by the expulsion of the Jews and the forcible conversion of the Moors deprived Spain of that uniqueness that had marked it throughout the medieval era.

As in his previous volume, Hillgarth stresses the diversity of the Iberian peninsula, arguing that unity was a concept fostered chiefly by propagandists laboring on behalf of Castilian claims to hegemony. In this, and in challenging other long-accepted interpretations, his work is stimulating and provocative. Castile and the Crown of Aragon remain the focus of his attention, while Portugal and

Granada are dealt with only incidentally. Even so, this remains a major study of late medieval Spain. There is an appendix on coinage, an extensive, annotated bibliography, an index, two maps, and two genealogical tables.

JOSEPH F. O'CALLAGHAN
Fordham University

DICK EDWARD HERMAN DE BOER. *Graaf en grafiek: Sociale en economische ontwikkelingen in het middeleeuwse "Noordholland" tussen ± 1345 en ± 1415* [Count and Counting: Social and Economic Changes in Medieval "Noordholland" between ca. 1345 and ca. 1415]. Leiden: New Rhine Publishers. 1978. Pp. xiii, 395. f 30.

This study of social and economic developments in the northern part of the county of Holland in the second half of the fourteenth century is based on data series generated from records of the counts' officials. These figures are supplemented by numbers from urban and religious institutions. The results are significant both because they add to a growing body of knowledge about Europe's economy in this period and because they reveal major structural changes in the Holland economy. The book's emphasis is squarely on the latter.

Discussion of demographic changes takes up nearly half the book. That is followed by a digression on coinage, then a treatment of wages and prices, and finally a section on production and trade. In each case there are introductory remarks, presentation of data, typically in tables and graphs, and then an evaluation. The last is generally short but precise and clear. The author has a tendency to present any figures and to rely on even the most sparse series. He also tends to ignore changes for which no data series exist. But with those caveats the rewards of the book are still great.

Dick Edward Herman De Boer is not able to establish conclusively how much the 1349-50 outbreak of plague affected Holland. But he does show beyond a doubt that there were "echo-epidemics," later outbreaks with high mortality leading to an overall fall in population. From 1369 to 1415 the proportion of the population living in towns rose, as farmers deserted poor land in the peat regions. Grain output fell both in those areas and overall, despite a rise in production in the coastal clay region. Arable land was no longer productive because of a series of floods in the 1370s and 1380s, caused by a rising water level and peat cutting, which lowered the level of the land. Real wages for peat cutters rose because of the demand for energy in the expanding towns' industries: textile manufacturing and especially brewing. Grain prices rose in Holland despite the fall in population since output fell

while demand, for example from brewers, rose. The solution was grain imports from northern France and then from the Baltic. By 1415 Holland had already changed from subsistence to specialized agriculture, had a much larger proportion of its population in towns, and had come to rely more heavily on internal and international trade.

De Boer claims all too often throughout this dissertation that his work is only a start and a base for further research. That is bothersome, but more serious is his insistence on repeating data while paying all too little attention to what is an interesting story. The extremely short conclusion does not do justice to the product of the research. The structural changes, the shift from traditional agriculture to the relatively modern sectors of trade and manufacturing that De Boer identifies, were in fact the basis for the long term growth of the Dutch economy. He is able to place the origins of Dutch success precisely in the second half of the fourteenth century. In addition it is fortunate that De Boer has not stopped with just the publication of his work but also has taken an active part in distributing this book, all to keep the price under control.

RICHARD W. UNGER
University of British Columbia

HELGE SALVESEN. *Jord i Jemtland: Bosetningshistoriske og Økonomiske studier i grenseland ca. 1200–1650* [Agriculture in Jemtland: Settlement History and Economic Studies in a Frontier Area, ca. 1200–1650]. (Det Nordiske Ødegårdsprosjekt, number 5.) Östersund, Sweden: Wisénska Bokhandelns Förlag. 1979. Pp. 187.

Scandinavian historians are currently involved in an ambitious project to study the “abandoned farm” phenomenon in all Nordic countries (*Det nordiske ødegårdsprosjekt*, DNØ) in an effort to understand the late medieval agrarian crisis. The first volume of the series (*Nasjonale forskningsoversikter* [National Research Surveys] [1972]) announced the goal of examining comparatively the settlement patterns from 1300 to 1600 through in-depth studies in the various countries, culminating in a comprehensive synthesis. So far, three studies have appeared, dealing with areas in Denmark, Sweden, and Norway. In this volume the Norwegian historian Helge Salvesen treats the Swedish province of Jemtland. Since Jemtland belonged alternately to Norway and Sweden during the Middle Ages and became permanently attached to Sweden only in 1645, this assignment makes good sense. For more than a century Norwegian historians have sought economic answers to explain their lack of political independence since late medieval times. More than other Scandinavian historians they have developed

methodological finesse and arrived at far-reaching conclusions in this field. These merits Salvesen brings to his own work.

Jemtland is of special interest because it was the only high plateau in Scandinavia with a relatively dense population since prehistoric times. The combination of height and latitude made it susceptible to even slight changes in economic patterns. Since the Black Death did not reach Jemtland, the effect of the plague, which is often given the greatest importance in accounting for the late medieval agrarian crisis, can be eliminated here. As established by the DNØ guidelines, the book is largely descriptive. One chapter deals with nature, climate, and resources. Another, which comprises 60 percent of the study, is devoted to an exhaustive and methodologically astute analysis of sources in three (out of forty) parishes. The key elements are Swedish tax records and tithe lists from the second half of the sixteenth century, which provide the oldest complete evidence of settlement. From here Salvesen works backward and examines medieval charters, place names, and archeological remains. In a concluding chapter he estimates the ratio of abandoned farms in Jemtland to be 36 percent in the mid-sixteenth century and argues that, while the process started here as elsewhere in the late fourteenth century, recultivation did not pick up until the first half of the eighteenth century. He sees this as a conscious change during the sixteenth century from intensive farming, necessitated by the population increase during the High Middle Ages, to extensive agriculture, where the “abandoned” farms were incorporated into existing agricultural units but were used only for the grazing of animals. The absence in Jemtland of the influence of both the noble class and the state, which elsewhere in Scandinavia encouraged recultivation, made it possible for the farming community to enjoy the labor-saving device of mixed husbandry for two hundred years. The book suffers from some repetition and lacks an index, but a Swedish glossary is provided.

JENNY JOCHENS
Towson State University

ARNO BORST. *Mönche am Bodensee, 610–1525*. (Bodensee-Bibliothek, number 5.) Sigmaringen: Jan Thorbecke Verlag. 1978. Pp. 584. DM 68.

“Gäbe es Brüderlichkeit ohne Brüderschaften?” (p. 389). This rhetorical question summarizes Arno Borst’s thesis. Superficially, this book is deceiving, appearing to be a regional history of monasticism, told through a series of biographical sketches. Actually, Borst has used selected monastic and religious houses to write a history of the interaction between the evolution of monastic communities and

the development of clerical and lay societies in the region around Lake Constance. Using a clear style unimpeded by jargon, Borst succeeds brilliantly in conveying the personalities and settings of his narrative.

Conflicting forces shaped monastic communities. They strove for separation from the day-to-day world, although the degree of their isolation and the means by which they attained it varied considerably. Against this segregation of themselves, the monks served their neighbors, both within and outside the cloisters, in a variety of ways that Borst traces ably. All monastic communities had poverty and spiritual autonomy as principles, but they had to support themselves. Whether they built up a complex of landed estates or participated in an urban market, their need for an economic foundation was another source of interaction with the outside world. Monasticism around Lake Constance originated outside the region and was repeatedly influenced by new movements from without, but regional power structures shaped the particular communities. Monastic communities were always on pilgrimage, moving not spatially but historically. They were in quest, shaped by a dynamism that prevented them from becoming static.

Borst divides medieval monastic history into four periods, the first two of which are clearly distinct, the latter two, in this reviewer's opinion, more arbitrary: From the arrival of St. Columban until the eleventh century, Benedictine monasticism arose within the Frankish and Ottonian aristocratic churches. In the later eleventh and early twelfth centuries, the Gregorian movement channeled monasticism into the reform of the clerical church. From the early twelfth century onward monasticism sought to shape the church of the laity. After ca. 1300 it found itself in the church of the burghers.

The eighty-four photographs are well chosen and fully described, but the format of the book unfortunately restricts their size. Perhaps to compensate for the space the pictures require, the publisher uses an exceedingly small typeface, which this reviewer found hard on his eyes. Borst argues that St. Francis of Assisi intended to found a brotherhood of laymen, without fixed convents or clerical structure, both of which were subsequently imposed on his movement, an interpretation with which this reviewer sharply disagrees. On the other hand, the bibliography, for example that on the Franciscans, is adequate and relatively recent.

Mönche am Bodensee is an original study of medieval monastic communities, a book whose importance extends far beyond its regional limits. No other historian has shed as much light on the monks in their physical and social environment.

E. RANDOLPH DANIEL
University of Kentucky

LAWRENCE G. DUGGAN. *Bishop and Chapter: The Governance of the Bishopric of Speyer to 1552*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 62.) New Brunswick, N.J.: Rutgers University Press. 1978. Pp. xv, 298. \$19.50.

The great difficulty in medieval history is that there is so much time to cover and so few documents to cover the historian's nudity. Thus, any attempt to study a minor but important institution such as the bishopric of Speyer over a long period of time is fraught with dangers. This book by Lawrence G. Duggan is more remarkable for the many pitfalls it has avoided than for the occasional unwarranted generalization. It is a study neither of the bishops of Speyer nor of the bishopric but rather of the evolving role of the chapter and its relationship to the bishops.

The author's basic assumption, stated in the introduction, is that "the cathedral chapter . . . in time became the ordinary, functioning 'parliament' of the principality" (p. 7). This is the study's main interest. Even though it does not succeed in proving this hypothesis for the period under consideration (1090–1552), it sheds a new and interesting light on both the governance of bishoprics and on the nature of representative institutions.

Chapter one describes the period of ascendance of the chapter, 1101–1272. Its principal interest lies in its treatment of the increasing power of the chapter as a process of local history, influenced but not dominated by the great movements of canon law and the conflict between emperor and pope. Although more might have been made of Speyer's very special position under the Salian emperors (the great cathedral itself is a symbol of imperial politics), the author's approach shows convincingly that the implementation of canon law relative to the chapter's position between 1140 and 1234 was not a foregone conclusion: it succeeded because in bishoprics like Speyer social and economic relationships were conducive to its implementation. The study strikes an interesting balance between explaining the developing relationship of bishop and chapter in terms both of general developments and of the small local problems that tended to add up to the big headaches in the provincial atmosphere of the bishopric.

A careful discussion of the electoral capitulation of 1272 follows in chapter two. This is in many respects the lynchpin of the argument; Duggan interprets it essentially as a modified form of *Herrschaftsvertrag* such as were concluded between temporal princes and their estates in the late Middle Ages. In this he is on a weak ground, as the wide disparity of cited examples, ranging all over Europe and over at least five hundred years, shows. The interesting lo-

cal details that may in fact go a long way to explain the document, unusual as it is, would probably have borne somewhat greater development. As the following chapter shows fairly clearly in describing the crises that befell the bishopric between 1272 and 1396, a major part of the struggles between bishop and chapter concerned a conflict between local and universal issues, with the chapter representing the economically well-founded local interest in which the increasing tensions with the citizens of Speyer, the financial management of the churches' property, and the efficiency of the courts played an absolutely central role. As Duggan points out, the background of the bishops (until 1396 generally from higher nobility and thereafter more often from the lower) was a decisive factor in their relationship to the bishopric and the chapter. In the latter period, discussed in chapter four, the bishopric was under the tutelage of the Elector Palatine. Although this diminished its standing, it obviously also contributed to peaceful and slightly more prosperous conditions after over a century of hardship.

Duggan offers a convincing picture of the way in which the chapter helped steer the bishopric through troubled times, often in spite of the bishop. But does this make a case that the chapter constituted the "parliament" or "diet" as he variously claims? Here he makes an argument by analogy; unfortunately, his ideas are too strongly influenced by concepts derived by others from fifteenth and sixteenth-century evidence, concepts hardly applicable to provincial Speyer in the thirteenth and fourteenth centuries. Otto Brunner is mentioned, but the lessons of his important work, *Land und Herrschaft* (1943), are not really absorbed; and Eugen Rosenstock-Huussy's seminal *Königshaus und Stämme* (1914), which deals with the rise of the principle of territoriality in the late twelfth century, is not mentioned at all. On the other hand, Duggan cites Max Weber on the theory of representation with respect to medieval Speyer. Whatever the merits of Max Weber, his knowledge of medieval history was rudimentary at best.

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RICHARD KIECKHEFER. *Repression of Heresy in Medieval Germany*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1979. Pp. xiv, 161. \$12.00.

This book deals with ecclesiastical and secular suppression of heresy in Germany from 1000 to 1520. Most of the work, however, is about the beguards, beguines, free spirits, and Waldensians of the fourteenth century.

Richard Kieckhefer argues that the inquisition as a formal institution never existed in Germany—or,

for that matter, anywhere else in Europe before the sixteenth century. Against Henry C. Lea he claims that episcopal or secular authorities rarely if ever challenged the jurisdictional rights of the papal inquisition. The reason for the ineffectiveness of the papal inquisition in Germany, 1311–1520, lies not in local opposition but rather in the structural defects of the papal inquisition itself. Insufficient staff, ill-defined procedure, and lack of direction from the curia explain its misguided attacks on fringe heretics. There is no need to assume political or economic motives by emperors, bishops, and townsmen to account for the arbitrary actions of the papal inquisitors. Regional powers resisted the outside inquisitors only when the latter exceeded their mandate. Ordinarily, the papal agents could count on local support in their repression of heretics. During the fifteenth century the inquisitors wasted their time hunting witches, political dissidents, and harmless fanatics; Waldensians and Hussites escaped. In contrast to the bungling papal inquisition, local episcopal judges often mitigated the effects of dangerous heresies.

Kieckhefer's approach holds up well in his treatment of fourteenth-century Germany, the most original part of the book. Historians may dispute some of his arguments for the nonpolitical motives of episcopal-municipal authorities, but his general conclusions about local operations against heresy are perceptive and convincing. He usually avoids socioeconomic interpretations, and his stress on the popular backing for antiheretical measures nicely complements his *European Witch Trials* (1976), which demonstrates the popular fears of sorcery.

The author unfortunately tries to make his brief (112 pages of text) study imply too much. His book hardly renders an "anachronism" (p. 10) of the term "medieval inquisition." It seems contrived to make Germany "typical" of "medieval repressions" (p. 10) and France and Italy "exceptional." In his sketchy discussion of the fifteenth century, he blames the lack of a papally controlled inquisition for the "ineptitude" (p. 110) of the inquisitors. If Germans did not perceive heresy as a major threat, does it make sense to have the papal inquisitors "dissipating" their energies against "marginal" heretics? But inconsistently the author would also have the antiheretical sentiments of most Germans make them disposed to assist papal agents. It seems unnecessary, even irrelevant, to speak of institutional inadequacies to explain why the fourteenth- and fifteenth-century popes did not intervene more vigorously in Germany. Obviously (as the author is forced to admit), the deficiencies of organizational machinery is a result, not a cause, of papal inaction. Perhaps it is true, as Kieckhefer contends, that the papal inquisition's very weakness explains the relative absence of jurisdictional conflicts between papal inquisitors and episcopal-municipal compe-

tence (p. 42). But surely the point at issue is the reasons for the *papacy's* inability or unwillingness to stamp out the major heresies in Germany. Would anyone seek to account for the ineffectiveness of Charlemagne's administrative network by pointing to the "institutional weakness" of the *missi dominici*? At the least, this way of stating the problem begs the question. Finally, the author's determination to see in this study of medieval Germany a moral about the nature of institutionalized repression (pp. x, 112) seems better suited to "relevant" pop history and book club selections than to serious scholarship.

But these criticisms of the opening and closing sections of the book do not detract from its solid analysis of the fourteenth century. Kieckhefer has written a fine synthesis of the German inquisition. His important study complements the recent works on heresy by Lerner, Leff, and Patschovsky.

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CLAUDIA ULBRICH. *Leibherrschaft am Oberrhein im Spätmittelalter*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 58.) Göttingen: Vandenhoeck and Ruprecht. 1979. Pp. 327. DM 70.

Late medieval and early modern western Germany was famous as a land of fragmented authority where peasants could simultaneously owe allegiance to an array of different territorial, judicial, land, and personal lords. The southwest especially knew a peculiar form of lordship over persons (*Leibherrschaft*) or personal serfdom (*Leibeigenschaft*), the meaning and impact of which have remained unclear, especially for the fourteenth and fifteenth centuries. Claudia Ulbrich's book, once a dissertation for the University of the Saar, investigates this phenomenon in a region around the great bend of the upper Rhine, roughly from the southern Black Forest south through Basel to Solothurn. The author traces the rise and development of personal lordship in exemplary ecclesiastical, municipal, and noble lordships and then devotes her last chapter to a systematic statement of findings.

Personal serfdom constrained the free movement, marriage, and inheritance of peasants along the upper Rhine. Except for the rare exaction of labor services or death dues, however, lords achieved thereby little direct material gain, only a few hens paid each year in token of subjection. Ulbrich connects the origins and intensification of this institutional structure with the economic difficulties and territorial ambitions of late medieval lords, asserting that it served to control peasant labor, enforce other, more directly remunerative, rights, and extend authority over persons who inhabited the lands of other lords. With the subsequent growth in

the region of true territorial states, however, personal lordship could be enforced only within one's own territory. Lords thus rationalized their states by exchanging enclaves of land and people to make of personal serfdom by the late sixteenth century just a common status for their subjects. Ulbrich argues, therefore, that personal lordship had a significant, though indirect, role in the institutional and political evolution of the area but that the particular form and function it assumed in each tiny lordship depended more on local conditions than on the differences in lords' status around which her study was organized.

Ulbrich's is a thorough and well-documented work of scholarship in the German tradition of institutional and legal history. The evolution of personal servitude in the region is plausibly connected to the lords' economic problems and political ambitions. Yet the context and implications of the phenomenon remain less than fully developed. The author skirts the precipice of an ecological fallacy when she appeals to general and vague evidence of a late medieval agrarian crisis in southwestern Germany to motivate specific measures taken by individual lords to intensify personal lordship. At the other extreme, however, little concern is shown for similar socioeconomic questions in the complex situation around 1500 when personal servitude was an issue relevant both to territorial state-building and to the peasant demands of 1525. The peasants are, in fact, rather the missing link throughout this study, for the perspective is wholly that of their lords. The objects of *Leibherrschaft* are just that, objects, who receive only seven pages of cursory comment at the end. Ulbrich's conclusion that personal serfdom had in the later Middle Ages more significance than students of more recent periods have perceived is, therefore, convincing and useful but very limited. In retrospect it is not surprising but fitting that the book does not conclude; it simply ends.

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MODERN EUROPE

R. R. BOLGAR, editor. *Classical Influences on Western Thought, A.D. 1650-1870: Proceedings of an International Conference Held at King's College, Cambridge, March 1977*. New York: Cambridge University Press. 1979. Pp. xiii, 394. \$45.00.

This volume contains the papers presented at the Third International Conference on Classical Influences held at Cambridge University in 1977. It is the last of a trilogy that traces classical influences on Western thought from the early sixth century A.D. to the late nineteenth century. This volume covers the period from 1650 to 1870, during which the

classical heritage was diluted, distorted, redefined, adapted to political and ideological causes, and finally superseded. But through it all, as the distinguished contributors to this volume show, the classical heritage remained remarkably vigorous and fecund.

The book is divided into four parts. The first part, entitled "The Epilogue to the Renaissance," examines classical influences in the seventeenth century: the influence of Roman law on the emerging national legal systems (H. Coing), Neoplatonic influences on scientific theory (J. Mittelstrass), hermetic and cabalistic influences on philosophy (C. Vasoli), the Neoplatonism of Comenius (K. Schaller), and the Pyrrhonism of Pascal, Bayle, and Hume (E. D. James). What all of these papers show is that the intellectual culture of seventeenth-century Europe, although no longer dependent on classical thought, would have been impossible without it. By 1650, as R. R. Bolgar correctly observes in his intelligent introduction, "there was not much more that the classical heritage could immediately contribute to the progress of European civilization, or at any rate there was not much more that it could contribute in the old straightforward manner that had been favoured by the humanists. But it had transformed the intellectual life of Europe, and its reputation stood high" (p. 8).

Part two, entitled "Antiquity as Myth," deals with the adaptation of the classical past to the political and social discontents and rising revolutionary temper in eighteenth-century Germany (M. Fuhrmann), Italy (J. H. Whitfield), France (R. A. Leigh), and America (M. Reinhold). The outstanding piece in this section is "La formation de l'Athènes bourgeoise: Essai d'historiographie, 1750-1850," by N. Loraux and P. Vidal-Naquet, which shows how the revolutionary picture of ancient Greece advanced in the eighteenth century was gradually transformed in the nineteenth to serve reactionary causes and interests. What all of these papers show, however, is that eighteenth-century nationalists, political radicals, and discontented intellectuals, although they still respected classical antiquity, consulted and quoted classical writers merely to confirm what they already believed or in order to give their causes intellectual respectability.

Part three, entitled "Antiquity as a Source of Facts," contains papers on the classicism of Vico (G. Costa) and Adam Smith (P. G. Stein) and S. G. Pembroke's study of classical influences on several theories of the early human family conceived between 1770 and 1870. These papers trace the assimilation of classical culture to the modern interest in primitive man and the origins of civil society. For Vico and Smith, classical literature was valuable not only as a mine of information about primitive societies, but also as itself an expression of the gen-

eral culture of the primitive epoch. When Stein writes that "Smith treated Homer almost as a text of social anthropology" (p. 265), it becomes clear how important this new conception of classical culture was in the development that led from the romantic idealization of primitive man to the beginnings of a new science.

The papers in part four discuss classical influences on the English utilitarians (H. O. Pappé), Goethe (T. Gelzer), Thomas and Matthew Arnold (R. R. Bolgar), Nietzsche (U. Hölscher), and Renan (J. Seznec). These thinkers were still able to find inspiration in the classical, especially Hellenic, heritage, but they were no longer able, or even inclined, to accept it at face value. For them, the classical world was only one source of enlightenment among many but no longer a uniquely privileged source. Yet their ideal of a long-extinct world, characterized by spontaneity and harmony, was still potent enough to guide their efforts to revive it in a new and higher form. Their failure marked the end of classicism as a significant influence on Western thought.

This volume is not, and does not purport to be, comprehensive; nor does it add much in the way of new knowledge. What it does, and does very well, however, is to remind us of the remarkable vitality and longevity of the classical heritage and the wide range of purposes it served for more than two centuries.

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NANCY L. ROSENBLUM. *Bentham's Theory of the Modern State*. (Harvard Political Studies.) Cambridge: Harvard University Press. 1978. Pp. xv, 169. \$15.00.

Recent scholarship has demonstrated that Jeremy Bentham was an Enlightenment philosophe dedicated to the progress and improvement of the human species. His range of interests included linguistics, ethics, and behavioral psychology as well as jurisprudence, penology, and government; his work was designed to form a complete social as well as political system based on the principle of utility. It is Nancy L. Rosenblum's contention, however, that Bentham subordinated all to the primacy of statecraft and the exigencies of political authority (pp. 6-7). For Rosenblum, Bentham was, first and foremost, a political thinker; the principle of utility was designed to serve "rulers." Again and again, Rosenblum writes, "The chief claim Bentham advanced for utility was as a rationale for legislation" (pp. 7, 9, 38, 39, 50, 55, 58). Indisputably, much of Bentham's writings included political theory; that this is the be-all and end-all of Bentham's work is simply too large a claim, given the facts. Moreover, this

is a very limited study since Rosenblum relies predominantly on the Bowring edition of Bentham's *Works*; the enormous mass of unpublished Benthamiana has not been investigated.

Yet the usefulness of Rosenblum's study lies in its introductory account of the political thought of a major Enlightenment thinker. Of particular significance is Rosenblum's chapter on Bentham's development of the social psychology of pleasure and pain as the ethical basis of laws in which the diversity and changing desires of the state's inhabitants had to be considered in the legislative process. There are also two strong chapters on Bentham's theory of sovereignty and international relations. Here Rosenblum argues that Bentham accepted the absolutist nature of sovereignty as part of his command theory of law but rejected its monarchical form and in its place substituted his theory of "popular sovereignty," which acts both to restrict and legitimize its power. His internationalism was predicated on the principle that a state insured its own well-being first, without doing injury to other states—a point that leads Rosenblum to conclude that Bentham, though an adversary of Machiavellian state egoism, might thus be placed within the "reason of state" tradition. Though debatable in some of the details, Rosenblum's book has opened up some interesting lines of discussion for Bentham scholars to confront in the future.

Perhaps the most debatable issue is Rosenblum's view that Bentham did not just accept the state—meaning the sovereign, territorial state of his own times—as "a norm of order," but, in her words, no one before him "has proselytized more fiercely on its behalf" (p. 2); no one has been more concerned with its political development, which Rosenblum calls modernization. As Rosenblum notes throughout her book, Bentham's state was one whose institutions and rationale for existence were diffused with the application of the principle of utility. Indeed, one might call Bentham's modern state the utilitarian state; in this sense, perhaps the equivalent of modernization is utility. When Rosenblum writes that Bentham opposed fanaticism, subjectivity in politics and all higher law, and "anti-legal ideologies," or that he divorced "reason and virtue" from the security that law provides (p. 54), or that his principle of the equal utility of states opposed universalism and "Enlightenment Cosmopolitanism" (p. 101), or that Bentham opposed philanthropic arguments and did not "advance the golden rule" (p. 104) in his theory of international relations, this could well demonstrate Bentham's utility and not necessarily his enthusiasm for the state as "a norm of order." It is useful to recall that utility, by definition, wars against meaningless phrases, catchwords, clichés, and abstractions that have little or no reference to specific human contexts. Ben-

tham was a thoroughly consistent utilitarian, and perhaps he "proselytized more fiercely" on behalf of this principle and the "greatest happiness of the greatest number" than he did for what Rosenblum calls the modern state.

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PETER MATHIAS and M. M. POSTAN, editors. *The Cambridge Economic History of Europe*. Volume 7, *The Industrial Economies: Capital, Labour, and Enterprise*. Part 1, *Britain, France, Germany, and Scandinavia*. New York: Cambridge University Press. 1978. Pp. xx, 832. \$46.50.

The sixth, seventh, and eighth volumes of *The Cambridge Economic History of Europe* are intended to cover modern developments not only in Europe but also in the whole "Western world" (including the United States and Japan together with European countries) "during and after their industrialization," starting in Britain in the eighteenth century. The sixth volume, previously published, was concerned with the "external parameters" of economic development: geographical expansion, demographic change, and technical progress. The present (seventh) volume focuses more narrowly on economic growth in the classical sense, dealing separately with the main "factors of production": capital, labor, and entrepreneurship. These factors are examined country by country, with national experts writing on each specialist topic, prefaced by a brief theoretical chapter from Solow and Temin interrelating these "inputs for growth."

The depth of specialist analysis is such, however, that this volume has had to be divided into two parts, each requiring a book of very considerable size, yet even so not covering the whole of Europe. Part one (here under review) deals with Britain, France, Germany, and Scandinavia, part two with the United States, Japan, and Russia. The inclusion of the United States and Japan and the exclusion of several important European countries inevitably raises doubts as to whether this can really be regarded as an "Economic History of Europe." Can it, with Russia and Japan included, even be meaningfully considered an economic history of the "Western world"? It is, in fact, neither but is, as the main title of this and the forthcoming eighth volume indicates, a collection of studies on the most developed "Industrial Economies" of the modern world, starting from a European core.

That being understood, this particular book provides an admirable analysis of the contributions and interrelationships of labor, capital, and entrepreneurship in the development of modern industrialized economies. With such well-known scholars as

Feinstein, Pollard, and Payne in Britain, Lévi-Leboyer, Lequin, and Fohlen in France, Tilly, Lee, and Kocka in Germany, writing expertly in their own specialist fields, and with Hildebrand dealing more comprehensively with Scandinavian developments, this book provides rich sources of information and illuminating interpretations, combining both quantitative and qualitative aspects, in what can only be regarded as a triumphant vindication of the editors' classical or neoclassical approach to analysis of economic development. The specialized investigation of factors of production is thoroughly justified by the insights provided into capital formation, investment, and financial institutions; into labor supply, recruitment, training, and industrial relations; and into entrepreneurial innovation and managerial functions, together with business structure and strategy.

The periods covered are not the same for each topic or country, some being confined to the classic period of the first industrial revolution in the eighteenth and nineteenth centuries, others continuing well into the twentieth century and up to recent years. Nevertheless, with their common approach and with such close economic, social, and cultural similarities between the European countries dealt with in this book—despite variations in factor endowments, institutions, and chronologies of development—the pattern that emerges is strikingly uniform, emphasizing the common features of industrialization, as against the differences or special features usually displayed in traditional national histories. Not only are we all “Europeans” nowadays, it seems, but we have always been, at any rate in our basic economic characteristics.

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ARTUR ATTMAN. *The Struggle for Baltic Markets: Powers in Conflict, 1558–1618*. Translated by EVA GREEN and ALLAN GREEN. (Acta Regiae Societatis Scientiarum et Litterarum Gothoburgensis, Humaniora, number 14.) Göteborg: Kungl. Vetenskaps- och Vitterhets-Samhället. 1979. Pp. 231. 50 KR.

Artur Attman, whose splendid work of 1944 on the trade at Narva in the sixteenth century immediately became a classic, should have left superb alone. This book will add nothing to his reputation and very little to Baltic historiography. For the specialist it is totally redundant. To the general reader, new to Baltic history, it will convey much interesting and even detailed information not easily available in English. But even the nondemanding reader will be distressed by the clumsiness of the language and upset by obvious errors. The translators, Eva Green and Allan Green, did not do a

good job; for example, “kapers” were not “pirates,” the “utliggare” of the Swedish navy off Narva were not “outriggers,” and the word “medlarna” means mediators not negotiators. The footnoting, aside from being excessively compressed, is carelessly done with items cited sometimes having no relevance to the text. The bibliography, although it is extensive, leaves out important works.

The main body of this book, and indeed it is little influenced by input from more recent publications, is copied directly from Attman's earlier writing. Paragraph after paragraph, chapter after chapter, is almost word for word the translation of the classic, *Den Ryska Marknaden* of 1944. The concluding, much briefer, portions are from his 1949 article on Stolbova. Since Attman, in his originals, could hardly have said it better, the decision simply to copy is understandable. Unfortunately, much is also left out, the gaps are only partially bridged with new text, and what we have is an inferior version (in English) of a good thing (in Swedish).

The subject matter is, of course, not at fault. It is as fascinating as it is many faceted. The Swedish effort to control trade in the eastern Baltic during a period of great flux, 1558–1617, was facilitated by many (sometimes quite extraneous) developments, complicated by others, and bitterly opposed, constantly or sporadically, by various combinations of Russians, Danes, Hanseatics, Poles, English, Dutch, Livonian Germans, and others. The melee was a military-political struggle for economic advantage for military-political advantage for . . . and Attman is master of the detail as well as the general scene. Unfortunately, as suggested above, in this book much of the detail has been dropped while the overview does not come through. If this is a new effort by the Swedes to conquer a larger market (in this case for history books), they would have done better not to adulterate their goods.

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ERNST L. PRESSEISEN. *Amiens and Munich: Comparisons in Appeasement*. Boston: Martinus Nijhoff. 1978. Pp. x, 153. f 32.50.

“Comparative history,” Ernst L. Presseisen tells us at the start of this little book, “will not attract those who insist on the uniqueness of each historical event.” To the “open mind that is willing to give it a try,” however, the comparative method “offers exciting possibilities,” indeed, the “best chance to go beyond history as mere chronology and to arrive at those comprehensive conclusions that evidence plus insight allows.” In fact (and this is Presseisen's purpose), when applied to the historical phenomenon of appeasement, the comparative approach “pres-

ents unique opportunities" for understanding causes, motives, and structure.

With open mind, then, and willing to give it a try, the reader follows Presseisen into his comparison of two episodes of British foreign policy: the deal with Napoleon that produced the Treaty of Amiens in 1801 and the appeasement of Hitler that culminated at Munich in 1938. It is immediately a path of confusion. The object before us is to chart the similarities of two revolutionary ages, two epochs of explosive, aggressive "ideologies," two eras of "mass participation in politics and war." At "first glance," Presseisen remarks, "these periods do not have much in common": the French Revolution "universal in its 'progressive' or 'liberal' character"; the "Nazi revolution" that is reactionary and racist. But somehow they fit together, as do Bonaparte and Hitler, who are described alternatively as "the embodiment of revolutionary storms" and as "autocrats." As for the ideologies of the two periods, our author-guide manages a dazzling integration. In the 1930s, he says, "the ideological picture is complicated by the simultaneous strength of communism. In fact, nazism and communism should be compared as a single ideological factor with jacobinism in order to appreciate the revolutionary character of both periods."

This reader, for one, parted company with Presseisen exactly here, at his introductory "Point of Departure." He has a story to tell, but it has to be winnowed out of the artificialities of the "comparative method." The only possible comparative focus is upon the British elites, in both periods men of limited vision and abilities, negotiating from economic weakness and frightened by popular unrest at home, desperate to make "peace at any price" with the foreign leaders whom they *perceived* to be respectable counterrevolutionary forces. (Napoleon in fact consolidated the "liberal" revolution—minus its nonessentials of political liberalism; Addington misread that, as Chamberlain, differently, and in the specificity of his own historical age, misunderstood Hitler.) The evidence for all this is in Presseisen's pages, but one has to search for it, extract it from his muddling together of Jacobinism, Bonapartism, socialism, communism, and nazism. The alternative to comparative history, Presseisen thinks, is "historicism"; it is an absurd choice, but in the circumstances that he offers, historicism wins.

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JAN ROMEIN. *The Watershed of Two Eras: Europe in 1900*. Translated by ARNOLD J. POMERANS. Middletown, Conn.: Wesleyan University Press. 1978. Pp. xxxviii, 783. \$25.00.

Congratulate the Wesleyan University Press for publishing this long work, which at first glance ap-

peals neither to specialists nor generalists. Specialists will find their focuses dispersed over too broad a canvas; generalists will lose their way in the myriad of detail that Jan Romein presents in each of his chapters. And yet the book is a treasury of illuminating fact and perceptive commentary.

This Dutch historian was a controversial personality all his life. He frequently polemicized against Johan Huizinga, whom he considered his intellectual mentor, and Pieter Geyl. He termed himself a Marxist, yet in 1927 was expelled from the Dutch Communist Party. His firm belief that history could only be understood in terms of dialectical contradictions was ironically borne out in 1949, when he was denied a visa to America because he was a "Communist" while the party still denounced him as a renegade. He died in 1962 fully involved with this study. His wife and several colleagues completed parts one and two, which constitute this book. Part three, intended as the theoretical culmination of his life work, was fully conceptualized in his mind, but unfortunately nothing had been committed to paper before his death. So the book remains incomplete but still impressive.

Romein's dialectical approach was more than Marxist; he developed it as an esthetic and dramatic principle. He saw "no forward movement without a return, no change without resistance, no oppression without the pursuit of freedom, and no progress without the risk of being overtaken by those who have lagged behind" (p. x). He tried to overcome the barriers of specialization and sought to understand this significant historical era by a three-dimensional comprehension of all facets of human thinking and endeavor. The Marxist pattern served as a guide but not as a strait-jacket. Ultimately he sought to establish a common human pattern by bringing in major elements of non-European history in order to understand better that crucial era when Europe was dominant in the world but not alone in it.

Romein commanded a wide range of bibliography, including seminal works of the diverse areas he studied. Yet he failed to list new works of his later lifetime that might have influenced his conceptions and methods: Henri Hauser, H. Stuart Hughes, J. R. von Salis, Gerhard Masur, and others. As an example of these limitations, see his chapter "Berlin to Baghdad." His views of imperialist economics and diplomacy were dated even in the context of his own life span. There are major references to works by Hallgarten, Helfferich, J. B. Wolf, and lesser items. But lacking are references to major works on international capital movements (Feis, Staley), German public enthusiasm (Dehn, Jäckh, Rohrbach), relations with Turkey (Earle, Holborn, Mühlmann), and recent diplomatic history (Langer, Chapman). The narrative on this subject

is sprightly, but the explanations are simplistic and the treatment is necessarily somewhat uncomplicated.

And yet, despite such evidence of omission, this is an original and intellectually suggestive book that is written in an engaging style. Most historians should be able to read it with stimulation and professional reward.

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RICHARD VAUGHAN. *Twentieth-Century Europe: Paths to Unity*. New York: Barnes and Noble, and Croom Helm, London. 1979. Pp. 261. \$21.50.

Richard Vaughan's *Twentieth Century Europe: Paths to Unity* opens with a list of eighty-eight abbreviations that include not only such central institutions of the Common Market as CAP (Common Agricultural Policy) and COREPER (Committee of Permanent Representatives) but also more peripheral European organizations such as JET (Joint European Tours), a nuclear research project, and FIFA (Fédération Internationale de Football Associations). The index contains even more of the abbreviations with which the text abounds, such as Coreunet (a cipher network), the aptly named COST (Committee on European Cooperation in the Field of Scientific and Technical Research), and the happily abortive Fritalux (customs union of France, Italy, and Benelux). The curse of the acronym lies heavy on the author who attempts to write a synthesis of the history of the European integration movement that is comprehensive in its coverage.

Vaughan's short book outlines the antecedents of the movement to limit the power of the European nation-state from the thirteenth to the nineteenth centuries, assesses the reasons for the failures in the interwar years of such "false starts" as Pan-Europa and the Briand Memorandum, and demonstrates the genuine progress toward supranationalism made since 1950. His excessive coverage of minor organizations and writers does, however, distract the reader from the significance of the work of the European Communities and creates the same ennui that now affects the general public in Europe whenever integration is mentioned. There is a certain advantage, however, in Vaughan's approach. His coverage of the federalist organizations and of the vast federalist literature provides evidence of the widespread public readiness for concrete moves after the Second World War to restrict the powers of national government. Without the wartime writings of federalists like Ernesto Rossi and Henri Frenay or the earlier work of writers like Gaston Riou, the proposals of Robert Schuman and Jean Monnet would have seemed far more revolutionary.

Vaughan's most unusual departure is to devote detailed analysis to the Scandinavian countries, whose cooperation he traces from the Norway-Sweden customs agreement of 1825 to the difficult adjustment of the Nordic Council to Denmark's membership in the Common Market; and he demonstrates that Nordic cooperation has not been so successful as to form a model on which Western Europe might have shaped its own integration.

Vaughan fails to set the integration movement adequately within the context of Cold War rivalries. In particular, he does not assess whether the European integration movement placed Western Europe more firmly in the American orbit or was a successful attempt to establish a counterweight to American influence. He also ignores the stimulating attempts of American political scientists, such as Carl J. Friedrich, to assess the extent to which integration has brought about a new form of political and social community in Europe. The book is useful as a form of textbook giving an introduction to the basic events of the integration movement. It does not claim to be a piece of research. It would, however, be greatly improved if the author had provided footnotes or, at the very least, a bibliography.

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VOLKER METTIG. *Russische Presse und Sozialistengesetz: Die deutsche Sozialdemokratie und die Entstehung des Sozialistengesetzes aus russischer Sicht, 1869-1878*. (Politik- und Gesellschaftsgeschichte, number 4.) Bonn: Verlag Neue Gesellschaft. 1979. Pp. 476. DM 46.

The author of this work seeks to define the "image" held by the Russian press toward the new German Empire and to understand, in particular, its response to the evolution of the German Social Democratic Party from its foundation to Bismarck's antisocialist legislation of 1878. The author demonstrates that the Russian press was slow to grasp the significance of the working-class movement, in part, at least, because of a belief that it was irrelevant for Russia where "a poverty of property owners" prevailed. In any event, the Russian press was preoccupied for the most part with domestic matters, and when it did report events in Germany, it tended to do so on a day-to-day basis with little inclination to examine the larger forces that were shaping German society. Those efforts by the press to be analytical tended to be shallow, and in the early part of the period the press perceived a phenomenon such as "ultramontane fanaticism" as the chief antisocial evil. In the wake of the Polish insurrection of 1863, in which Russian nationalists professed to see the fine hand of the Vatican, this was a view that was easy for the Russian press to accept.

But in 1878, when Bismarck began to use attempts upon the life of the German emperor to push antisocialist legislation through the Reichstag, the Russian press began to follow German affairs more closely and to view them against the background of rising terror within Russia. The situation proved particularly agonizing to Russian liberals, who had to choose between unfettered terror or legislation that threatened their own freedom. This dilemma divided the Russian liberal press, and, as the controversy evolved, the Russian press as a whole came generally to reflect similar divisions within Germany. *Golos, S. Peterburgskie vedomosti*, and *Birzhevye vedomosti* adopted a "progressive" national-liberal posture; and *Vestnik evropy* pursued a "progressive" course "with democratic tendencies." These relative positions, according to the author, reflected the attitudes of these publications toward Russian domestic issues, although his tendency to resort to such unqualified political labels as "liberal" and "conservative" leaves this conclusion without firm foundation. These terms have meaning only within the Russian context and require considerable explanation to possess any real significance.

The treatment of the material in this study is excessively narrow. Two-thirds of this lengthy volume treats a few months in 1878 in tedious detail. Much could have been summarized and characterized, making the work both more readable and more focused. In view of the author's unsurprising conclusions, one regrets the editorial decision not to require substantial pruning of the manuscript. Perhaps the reason for not doing so lies in the fact that this work was a dissertation at the University of Bonn, appearing here as volume four in a series published by the Friedrich-Ebert Foundation, and is cheaply produced in offset typescript. The editors may also be faulted for the fragmented and choppy structure, owing to the author's constant use of subtitles rather than transitions. The contribution this work makes to our understanding of Russia is small, and it does little to illuminate the history of Germany.

FORREST A. MILLER
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GERHARD SCHREIBER. *Revisionismus und Weltmachtstreben: Marineführung und deutsch-italienische Beziehungen 1919 bis 1944*. (Beiträge zur Militär- und Kriegsgeschichte, number 20.) Stuttgart: Deutsche Verlags-Anstalt. 1978. Pp. 428. DM 58.

One of the primary functions of the "new" military history is to integrate strategic and tactical considerations into the broader spectrum of social, economic, political, and diplomatic history. This book admirably achieves the goal. Gerhard Schreiber's

analysis of German-Italian naval relations rests upon an impressive documentary basis: the files of the Federal-Military Archive in Freiburg, the Foreign Office Archive in Bonn, the Naval Historical Branch of the Ministry of Defence in London, and especially the Archivio Ufficio Storico della Marina Italiana in Rome. Nor is Schreiber afraid to tackle the plethora of books dealing with Germany's role in the Mediterranean in particular and in the Second World War in general.

A major theme of the work is revisionism. During the early years of the Weimar Republic, German naval officers were already planning on armed alteration of the Versailles Treaty and for this reason rejected the politics of Stresemann, even though the latter was not averse to revisionism. Schreiber accurately describes the naval officers' early acceptance of National Socialism and their almost sycophantic loyalty to the Führer under Admirals Raeder and Dönitz until the last days of the war, if not beyond.

Schreiber also stresses that the navy was firmly committed to a "final solution" of the English question, even though the term *Endlösung* was not applied to Britain until 1939. At a crucial meeting at Friedrichshafen with the Italian Chief of the Admiralty Staff, Domenico Cavagnari, Raeder depicted the coming conflict with London as a "life-and-death struggle." It is also interesting to note, in light of Raeder's later claims in behalf of his so-called Mediterranean strategy, that at this meeting he bluntly informed the Italians that not even the seizure of Egypt and the Suez Canal could displace England from the Mediterranean; an inferior English fleet would defeat any superior Italian fleet "on the basis of their experience and tradition" (pp. 173, 174).

Close relations with the Italians did not come readily. Memories of Italy's defection from the Triple Alliance in 1915 died very slowly in the German navy, and it was not until 1926 that naval officers, in stark contrast to political leaders, embraced the Fascist regime in Rome—while actively working against democracy at home. In the process, the fiction of the "unpolitical" officer was once again exposed. Schreiber concludes that the German-Italian naval cooperation can best be depicted as "an alliance to negate the existing," that is, to drive England from the seas (p. 393).

The author asserts that in the absence of sufficient means with which to conduct a war at sea, German naval planning constituted little more than mental gymnastics (p. 12). Yet this "dreaming in continents," as General Franz Halder depicted it, reveals an incredible degree of continuity from the 1890s (Tirpitz) through the First World War (Hötzendorff, Levetzow, Trotha) and the victorious period of the Second World War (Fricke, Schniewind, Wagner). And it did not end even here. In Septem-

ber 1944, long after Hitler and his military paladins had conceded that the war was lost, Admiral Alfred Saalwächter penned a memorandum detailing Germany's needs for the next major war. He called again for German naval bases in the Netherlands and Norway, a "protectorate" over much of Belgium and France, coaling stations on Iceland and the Faeroe Islands, a "missile-launching base against America" on Greenland, advanced naval depots on the Azores, Cape Verdes, and Canary Islands, and a Central African colonial empire. In short, not even the horrendous devastation of 1944 could deter the fifty-year-old Machiavellian dreams of Germany's naval leaders.

Finally, Schreiber's work fits well into the genre of historical writings we have come to expect from the Military-Historical Research Office in Freiburg, and especially from Manfred Messerschmidt and Wilhelm Deist. It is the hope of this reviewer and others that they will be given the freedom to encourage such sober reappraisals in place of the apologies and hagiographies penned since 1945.

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ROY DOUGLAS. *The Advent of War, 1939-40*. New York: St. Martin's Press. 1978. Pp. xiii, 167. \$19.95.

This is an excellent summary of fourteen fateful months. In March 1939—more precisely, between March 17 and 21—the government of Great Britain changed its course. It had come to believe that the only way to avoid a second world war was to serve notice on Hitler that future German aggression against certain states would be a *casus belli* for Britain; and a British guarantee was offered to Poland. On May 10, 1940 the elected representatives of the British people demonstrated their preference for Winston Churchill as their prime minister; and the reluctant war began in earnest that day. Important milestones as these events are, they are especially dramatic turning points in the history of Britain. This book is written from that perspective, even though the author demonstrates his spacious comprehension of European international relations. It is principally a diplomatic history, dealing with the relations of states and of their governments, because of the author's reliance on the recently opened British state papers of the period. In this respect, Roy Douglas is more judicious than Sidney Aster, who wrote that these documents contain the vital evidence that "finally" reveals the making of the Second World War; he is also more judicious than A. J. P. Taylor, whose often brilliant but frequently superficial views made him write that the German demands on Poland were not altogether unreasonable.

Douglas has an excellent comprehension of the realities of international relations, perhaps especially in his last chapter, entitled "Retrospect," where he writes that no statesman, including Hitler, "behaved in a manner rooted in ideology, when that ideology ran counter to perceived immediate interests"—an argument of which his illustrations are well taken.

My only criticism of this fine little book is the author's evident underestimation of the American factor. As early as 1939 behind many of the deliberations and actions of the British government stood the inclination of increasing dependence on the United States and the determination of ensuring American sympathies for Britain. This was a factor even when it came to marginal areas of British interest as, for example, in the British deliberation about how to assist Finland. During the visit of the royal family to the United States in the summer of 1939, the first British suggestions were made about turning over certain British bases in the western Atlantic to the United States. No diplomatic historian ought to ignore the American element, even before 1940, when its importance became overwhelming.

JOHN LUKACS
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FRANK G. WEBER. *The Evasive Neutral: Germany, Britain, and the Quest for a Turkish Alliance in the Second World War*. Columbia: University of Missouri Press. 1979. Pp. ix, 244. \$19.50.

Wartime Turkey was a den of intrigue. From 1939 to 1945 agents and ambassadors of every power, cause, and faith operated against one another without remission and without remorse in the salons and along the open boulevards of Istanbul and Ankara. Turkey was not officially a participant in the Second World War, and, by remaining elusively neutral, it became a prized plum dangling just out of reach of both sides—a tradition of defensive foreign policy in which the Turks excelled since the 1699 Treaty of Karlowitz signaled the beginning of the retreat of Ottoman power. From its vantage point astride the straits and its ability to resist almost any military invasion, Turkey shamelessly manipulated both the Allies and, to a far lesser extent, the Axis. By diplomacy alone, Turkey maintained its territorial integrity, ultimately milking the Allies for substantial lend-lease, even while denying its friends in Berlin a long-sought-for alliance with the Arab world.

Frank G. Weber's new book, *The Evasive Neutral*, is an odyssey that begins after World War I—a seemingly logical continuation of the author's earlier volume, the masterful *Eagles on the Crescent* (1970). Yet the Turkish republic that survived the

First World War bears little resemblance to the far-flung Ottoman Empire of Weber's earlier book—with one notable exception. Although Atatürk declared in his speech on the abolition of the sultanate on November 1, 1922 that "Sovereignty and supreme power are won by force," Turkey's future was based, as always, on diplomacy and its ability to play off the interests and demands of its suitors. After Turkey's diplomatic victory at the Montreux Conference in July 1936, which allowed it to refortify the straits, its suitors were Britain and France on one side and Nazi Germany on the other. The result was a fascinating courtship dance between foxes, which lasted nearly a decade.

Of particular strength throughout the book is the exposition of Berlin's Middle East policy, such as it was, which Weber unravels in meticulous detail. Against a backdrop of economic missions, political machinations, and agents crisscrossing Europe with secret proposals, the Germans simply could not decide whether to support the Turks (for their strategic geographical position and militant anti-Soviet potential) or the Arabs (who would be geographically invaluable in a giant pincer movement against Russia and able to erode substantially British and French military strength in the Levant). Clearly, the Wilhelmstrasse could not befriend them both, and therein lies the key to Germany's failure in the Middle East. Each approach by the Turks or the Arabs caused Berlin to agonize, vacillate, and ultimately rebuff the advance. When Ankara finally, though half-heartedly, declared war on Germany in February 1945, resurrecting its long-abused 1939 mutual assistance treaty with Britain and France, the decision was brought about as much by Germany's muddled policies as by Ankara's diplomatic duplicity and opportunism.

One marvels at Weber's ability, in slightly more than two hundred pages, to analyze the convoluted efforts of both camps to enlist Ankara in their global struggle, while the object of their attention was playing each for the highest stakes. At the same time, Weber never loses sight of the shifting rivalries within the Arab world—underscored by Turkey's opposition to the unification of the Arab Middle East and by the latter's fear of "Kemalist imperialism." *The Evasive Neutral* is compact and tightly woven and based on impeccable research. The inclusion of maps would have been welcome. Until the Turkish government opens its archives to scholars, a distant prospect at best, Weber's *Evasive Neutral* will doubtless remain the standard work on this subject.

ARNOLD KRAMMER
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HELEN FEIN. *Accounting for Genocide: National Responses and Jewish Victimization during the Holocaust*. New

York: Free Press, and Collier Macmillan, London. 1979. Pp. xxi, 468. \$15.95.

This is an ambitious work, as any attempt at "accounting for genocide" must be. Although the accounting has its flaws, it is also one of considerable importance and, therefore, bound to be controversial. Helen Fein takes on the problem of Nazi genocide from outside the much-explored German perspective to examine in detail the roles played in the Final Solution by non-German peoples, including that of Jews themselves.

The finality of that solution, Fein explains, "has diverted attention from the fact that in almost half . . . the states occupied by or allied to Germany, fewer than half of the Jews counted there were killed" (p. xvi). That various peoples within the Nazi orbit reacted differently to Hitler's extermination policies has long been known. Many collaborated willingly, even eagerly; some few resisted outright. Why reactions varied so widely and the effect of these variations upon the number of Jews finally murdered are the principal problems taken up in this book.

How were Denmark and Bulgaria, where most Jews were saved, different from Croatia, Slovakia, or Poland where most of them were killed? The answer, the author contends, is to be found only by seeing the states and peoples under Nazi hegemony as parts in a "macrosystem of international power" that embraced also those powers allied against Germany. Then alone is the conceptual net cast widely enough to draw in all the possible variables. To keep track of all these variables, Fein employs, as she calls it, "various sociological modes of knowing," including "inductive, historical analysis . . . a *verstehen* approach . . . and statistical analysis, employing cross tabulation and regression analyses" (p. 36).

The yield from so many "modes of knowing" is often no more than what should have been obvious from the outset: the degree of a people's cooperation with the Nazis was determined largely by the extent of their prewar anti-Semitism and the immediacy with which they were under SS control (p. 82). But there were exceptions and Fein's theoretical structure is useful in explaining them. In the Netherlands, for example, there had been relatively little anti-Semitism, and SS control was certainly less immediate than in Eastern Europe; yet three of every four Dutch Jews died in extermination camps. By contrast, in Rumania, where a virulent anti-Semitism had long flourished and where the government, frustrated by Nazi slackness, initiated its own slaughter of Jews, over half of them ultimately survived.

Fein accounts for such seeming anomalies by identifying variables such as protests from the churches, the effectiveness of resistance movements,

the formation of Jewish Councils (*Judenräte*), conflicts within the Jewish communities, calculations about the likelihood of ultimate Nazi victory, and international intervention. To varying degrees and in various combinations, all carefully analyzed, these factors either impeded or facilitated the Nazis' murderous intentions.

Occam's razor might usefully have been employed to trim away some of the theoretical undergrowth in this work. Explanation and exposition would have been enhanced by the resulting economies. Nonetheless, this book warrants a careful reading. It is the first synthesis of Nazi genocide from the non-German perspective. Among its virtues is the widely scattered specialized literature it brings to our attention.

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KATHARINE R. FIRTH. *The Apocalyptic Tradition in Reformation Britain, 1530-1645*. (Oxford Historical Monographs.) New York: Oxford University Press. 1979. Pp. vi, 281. \$26.00.

The concern of recent scholarship with millenarian ideas in Stuart England prompted Paul Christianson and Katharine R. Firth to explore the Protestant apocalyptic tradition out of which this millenarianism developed. Like Christianson's *Reformers and Babylon* (1978), Firth's work is a revised doctoral dissertation. The books cover much of the same ground, commencing with John Bale's identification of antichrist with the papacy and his focus on the fulfillment of prophecy in history. Unlike Christianson, Firth displays little interest in the impact of apocalyptic thought on political events, preferring instead to explore in learned detail the views of the principal apocalyptic authors, including Joye, Foxe, Knox, Napier, Broughton, Brightman, Raleigh, Hakewill, and Mede. Of these, Christianson ignores Joye, Raleigh, and Hakewill and provides token mention of Knox and Broughton but does demonstrate the impact of apocalyptic ideas on such radical critics as Prynne and Lilburne. Essentially, the studies are complementary and substantially in agreement. For a thorough exposition of apocalyptic ideology Firth's is the better book, though Christianson excels in linking the theories to radical protest.

Firth argues that the apocalyptic tradition was revived by the Lutherans, though the example of Münster made many chary. Calvin demonstrated restraint with respect to apocalyptic matters but accepted the historical revelation of antichrist in the papacy. In England the Protestant apocalyptic tradition was established by Bale, Foxe, and Knox. Influenced by Joachim of Fiore, Bale developed the

connection between prophecy and history. "From then on, the two most important characteristics of the Protestant tradition were a history of Antichrist and a periodization based on a succession of apocalyptic images" (pp. 248-49). Foxe, believing that the millennium had begun with Constantine, wrote his magisterial study of the Church partly to ascertain the fulfillment of prophecies. (Firth and Christianson demolish Haller's thesis that Foxe conceived of England as the elect nation.) The application of prophecy in history was continued by Knox, but Napier, influenced by Ramist methodology, developed exegetical techniques that led him to the discovery of propositions on which he predicted the future.

Firth demonstrates that Broughton, Brightman, and Mede refined the schematization of the past and thus the expectation of forecasting the future. Brightman was the first (in 1609) to refer to England as the elect nation, a concept that became increasingly credible in the context of the Thirty Years' War. In the 1630s Brightman was interpreted as a harbinger of millenarianism, though Hakewill and others resisted such views. Millennial ideology was influenced by the Continental writers Andreae, Alsted, and Comenius and by the Englishman Mede. As these ideas spread, the English began to expect a millennial kingdom on earth rather than the end of the world predicted by the older apocalyptic authors. The change partly resulted from a decline in the historical tradition by the postponement to a future date of prophecies previously thought to have been fulfilled in the past. Historical study thus lost some of its relevance and was replaced by the imaginative projection of prophecies into the future. The chiliastic spirit was fanned from the pulpit and kept alive in Parliament through fast sermons. Although the revolution with its chiliastic overtones collapsed, the apocalyptic tradition was still being explored at century's end by Issac Newton.

RICHARD L. GREAVES
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CONRAD RUSSELL. *Parliaments and English Politics, 1621-1629*. Oxford: Clarendon Press. 1979. Pp. xxi, 453. \$36.50.

Conrad Russell's study of the Parliaments of the 1620s has been long awaited and does not disappoint. It is, as we should expect, a direct attack on the "winning of the initiative" thesis of the late Wallace Notestein and his successors. It is also, more obliquely, a reaction against recent arguments that long-term social forces underlay the political conflicts of early seventeenth-century England: there are no Parliaments of rising gentry or aristocrats in crisis here.

Both the lengthy introductory chapter (on the general structure of politics and the nature of parliamentary business) and the succeeding ones dealing with each of the six sessions between 1621 and 1629 exhibit Russell's authoritative mastery of the subject. The book provides by far the best consecutive account of parliamentary politics during this critical decade. Russell's superb control of his evidence is immediately apparent in a marvelous little introductory "Note on Sources," but it infuses the whole book and is buttressed by an equally impressive knowledge of the intricacies of parliamentary procedure. With a sophisticated understanding of the realities of Westminster politics and of their interaction with the more provincial concerns of the members, Russell is able to weave a complex tapestry of court intrigue, misunderstanding, and frustrated expectations. Even during this decade of increasing legislative paralysis, he shows that Parliament's function as a law-making institution should not be entirely neglected. And although proceedings in the Commons inevitably receive more space, he pays due attention to the House of Lords whenever appropriate.

Arguments from the author's earlier articles are here extended and elaborated. Russell rejects both the "conflict model" underlying Notestein's work and the teleological approach (the search for origins of the civil war) that has preoccupied so many other historians from Gardiner to the present day. The committee of the whole, we discover, was not a procedural device to enhance Commons' independence; the Commons were always reluctant to use supply as a lever for obtaining redress of grievance (even appropriation of supply in 1624 turns out to be a court scheme); while in foreign affairs they "were not seizing the initiative: they were evading it" (p. 81). In fact, Russell argues, the real scene of political debate was not Parliament but the court, and parliamentary politics was thus in the main an extension of court intrigue. In 1621 the attack on monopolies was not a move by the Commons against the court but an attack on some courtiers promoted by other courtiers. Impeachment was simply "a tool used by one Councillor to attack another" (p. 15), a subject which belongs "not to the history of ministerial responsibility, but to the history of court faction" (p. 104).

Historians, Russell believes, have exaggerated the sharpness of the court-country division. Most M.P.s, he points out, had a foot in both camps, a dual loyalty to their king and to their "countries"; and he makes much of G. R. Elton's suggestion that Parliament was in essence the point of contact, the conflict-resolving mechanism between Westminster and the localities. There is no doubt that the tension inherent in this relationship explains a lot of the political instability of the 1620s, for much as

they might have wanted to, M.P.s found it increasingly difficult to serve both a king who needed money for war and constituents who did not understand his admittedly often inept foreign policy. The 1620s, Russell does well to remind us, was a war decade, and neither Parliament nor the local administrative system was equipped for this: the result was "functional breakdown." Sheer accident, he argues, has more to do with the conflict than ideological conflict or the remorseless working out of social change: accidents like the "failure in communication" (p. 137) that led to the 1621 Protestation or the nervous atmosphere created by the plague in 1625. Once again Russell finds no continuity between the 1620s and the 1640s and concludes that the nation was not "profoundly alienated" before the outbreak of the Scottish troubles in 1637 (p. 426).

Russell's deployment of this argument is impressive. But even this magnificent book does not tell the whole story. Russell provides what might be described as the "court" view of politics. He is, to be sure, sensibly aware that Westminster represents only the tip of an iceberg, that Parliament was to a large extent a mirror of events taking place elsewhere. He makes a bow to Derek Hirst's recent study of the expansion of the electorate but does not discuss the implications of the Commons' anxiety to encourage an electorate independent of court or aristocratic influence. Outside events such as the growing unpopularity of courtiers and the heightening of religious debate also receive rather short shrift: until 1629, he supposes, even the rise of Arminianism was primarily a court issue. The consensus model, throughout, is overdone. Many M.P.s were clients of Buckingham and other courtiers, true enough; and Russell has some admirably shrewd comments on the nature and limits of patron-client relationships. But the unwary reader might all too easily suppose that these ties of dependence—of Harley on Buckingham, Rudyard on Pembroke, and so on—are a complete explanation for the political behavior of the Commons. When, therefore, we find ourselves in a new political universe in the Petition of Right debates in 1628, a universe no longer dominated by the court and clientage, the transition seems abrupt and not fully explained. Russell's treatment of the great debates of that session has many merits: he provides, for example, his usual excellent coverage of the Lords, and analyzes the Commons' central core of ideas about law and liberties very effectively. But he misses much of the drama and the excitement that breathes through the great speeches such as those of Rudyard and Phelps on March 22. On Russell's assumptions the final 1629 session is bound to "peter out into futility"; its final, almost revolutionary scene of passion and violence, when the speaker was forcibly held

down in the chair, becomes a pointless anticlimax rather than a prelude to even more revolutionary events.

In Russell's view the Parliaments of the 1620s seem to differ little from the Parliaments of Elizabeth's reign. Yet there was court faction under Elizabeth, too, without Elizabethan councilors finding it necessary or convenient to impeach each other. The fact is that by the 1620s English society had changed, real differences had emerged over the meaning of laws and liberties, differences that were beginning to agitate people far away from Westminster. Russell wisely observes that the bonfires in the streets that greeted the king's acceptance of the Petition of Right may in the end be more historically significant than the document itself. His book is a brilliant analysis of how the politics of the 1620s looked to the court. But it all may have looked very different to the country.

DAVID UNDERDOWN
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JOHN H. PRUETT. *The Parish Clergy under the Later Stuarts: The Leicestershire Experience*. Urbana: University of Illinois Press. 1978. Pp. 203. \$10.00.

This study of the Anglican clergy in Leicestershire from the Restoration to the death of Anne is replete with quantitative studies of social origins, incomes, educational and geographical backgrounds, and political leanings. The text is studded with individual examples, fully treated, and the variety of practice and situation is given full play. There are diverting details about tithes, patronage, and the like, that make for a readable discussion, which, though a local study, gives us a representative, panoramic view of the life and times of later Stuart clergy.

The conclusions are both firm and cautious. The author must have been disappointed after so much labor to have found nothing really surprising. The steady decline of clerics of plebian origin, noticeable since the Reformation, continued throughout this period; educational standards rose; and, because incomes were tied through tithes to agricultural produce and prices were rising, the clergy benefited. So, economically, educationally, and socially the position of the established clergy rose.

Yet enthusiasm tended to dim. The diminution may have been the reflex of comfort. But the intransigent Restoration Settlement cut the Church off from Puritan intensity; and the Act of Toleration of 1689 ushered in a further decline in fervor. Zeal was insidiously defrocked by secularism. The impression remains that most Leicestershire clergy were conscientious. We know how many, like Goldsmith's *Parish Priest*, were "passing rich with forty pounds a year," but we cannot discover how many were "to all the country dear."

All the proper and necessary questions have been asked, from the vantage of the social historian. The daily rhythm of life, the quintessence of doctrines as they were understood, the texture of teachings, and the soul and spirit of life in the parsonage are not much canvassed. That great shift from sacramental to institutional Christianity and from strenuous atonement to tepid ethical theology is left untraced. The kind of research needed for this book, indeed, diverts one's mind from such issues. The research is meticulous. As a result we have a useful study that, by documenting the rise of the clergy, gives us a clue to their alienation from the lower social orders—the copyholders and landless laborers whose position was surely deteriorating. The divorce from the masses, however, was a matter of mind as well as of money and class. In this complex divorce we can see the seeds of the modern Anglican tragedy, where, in so many serenely beautiful country churches, the vicar intones evensong to empty pews and preaches on Sundays to his wife and the verger.

JOHN F. H. NEW
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JOHN BOSSY. *The English Catholic Community, 1570–1850*. New York: Oxford University Press. 1976. Pp. xii, 446. \$24.95.

In his first book after many articles, John Bossy offers an interpretation of English Catholicism from the beginning of the Elizabethan mission to the restoration of the hierarchy. The word "community" in the title is crucial; this is not primarily an account of an institution or a set of beliefs but of a group of people with common loyalties, common behavior, and a common relationship to the larger communities of Catholicism and England.

The work has three sections and a concluding chapter. Part one traces the mental and institutional adjustment of Catholics, particularly the clergy, to the end of the medieval Church and to the new situation in which Catholics were one of several religious minorities in a country dominated by Anglicans. Part two is a portrait of English Catholicism from 1600 to 1770. It discusses the nature of the gentry household and its domination of the community; clerical roles, structure, and finances; the community's size and distribution; and the role of worship and ritual. Part three treats the expansion of the community between 1770 and 1850 (from native English as well as immigrant Irish sources) and the decline of gentry domination—both developments whose roots Bossy finds in the early eighteenth century. Bossy sees, following the decline of gentry control, a period of conflict between lively congregational participation in worship and authority and several forms of clericalism.

This conflict ended in the victory of episcopally centered clerical authoritarianism, which the hierarchical restoration of 1850 both effected and symbolized.

This is an extraordinarily good book. Bossy has great knowledge of his subject and a very creative mind—indeed, sometimes the rapid succession of ideas compels readers to go slowly if they want to absorb everything. Bossy also has just the right degree of sympathy for his subject; he cares strongly about English Catholicism and approves of some tendencies within it and not of others, but his sympathies rarely compromise his objectivity.

One cannot mention all of Bossy's theses in a short review, but some of the more important may be summarized. Bossy sees post-Reformation English Catholicism essentially not as a continuation of medieval Catholicism but as a new entity created by dissatisfaction with the Church of England. In this it was like other Nonconformist sects, and Bossy's work is conditioned by his view that "the Catholic community ought properly to be considered a branch of the English nonconforming tradition" (p. 7). A corollary of this view is that the English Catholics after the launching of the mission were not a dwindling remnant of a formerly dominant church but a sect that had virtually no active members before about 1570 and whose history from 1570 to 1850 was basically one of growth.

In a book of such comprehensive scope and intense thought it would be surprising if a reviewer could find nothing at which to carp. I am not, for example, fully convinced by Bossy's statistical arguments on the size of the community (chap. 8), and his discussion of the large role played by upper-class Catholic women in the fifty years after 1570 (pp. 153–60) may dismiss too easily the fact that women may have been freer than men in their religious behavior because their choices had fewer unpleasant consequences. Others will find other points with which to disagree. But Bossy's overall argument is extremely compelling, and his book should be read by anyone interested in the history of Christianity in Britain or Catholicism since the Reformation.

ARNOLD PRITCHARD
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HILLEL SCHWARTZ. *Knaves, Fools, Madmen, and that Subtle Effluvia: A Study of the Opposition to the French Prophets in England, 1706–1710*. (University of Florida Monographs. Social Sciences, number 62.) Gainesville: University Presses of Florida. 1978. Pp. 97. \$5.50.

This impressive monograph, part of a revised dissertation (Yale, 1974), focuses on the English reaction to the French prophets who arrived in London

in the summer of 1706 and startled that uneasy metropolis by their agitated behavior and disturbing visions. Hillel Schwartz contends that, by studying the twenty-six critics who published responses to three of these *inspirés* between 1707 and 1710, "one can trace assumptions about religious behavior which are guides to early eighteenth-century beliefs about the nature of all human activity" (p. 5).

It is an ambitious undertaking and a useful, though admittedly incomplete, addition to the growing collection of books on millenarianism. In a heavily documented work influenced by a wide range of contemporary scholars, Schwartz has asked pertinent questions about the theories of human and religious behavior; the nature of criticism and the meaning of metaphors; the number, age, sex, and socioeconomic standing of the prophets and their followers; the dynamics of individual and group behavior; the medical explanations of insanity and enthusiasm; and the interplay between memory and time in late Stuart England.

In "Backgrounds," Schwartz explains how the French prophets came to a society that was already much divided over reason and religion. He attempts to link political and religious tensions in the age of Queen Anne to economic and social undercurrents but oversimplifies here and admits that his treatment is abbreviated.

The heart of the book is "The Critique," in which the author analyzes the responses to the prophets according to the categories of animal spirits, effluvia (vapors), and delusions. Diverse critics attacked the prophets for threatening the order and balance in society that had been emerging gradually from Cartesian, Newtonian, and Lockean postulates. In order to preserve the rational basis of human behavior and the body politic, critics felt compelled to advance natural causes to explain the extraordinary agitations of the prophets. By insisting that behavior was rational and that extraordinary behavior was caused by natural and physical unbalances, however, the critics were undermining the role of ritual in religion and destroying the distinction between religious experience and everyday activity; consequently, they were preparing the way for the evangelical revival and the reassertion of the spiritual dimension to worship and being.

This is a demanding and rewarding work of concentrated scholarship. One can overlook the occasional cursory argument by looking forward to what Schwartz promises to be a larger work on the French prophets.

THOMAS W. DAVIS
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ROBERT E. TOOHEY. *Liberty and Empire: British Radical Solutions to the American Problem, 1774–1776*. Lexing-

ton: University Press of Kentucky. 1978. Pp. xiv, 210. \$13.00.

British radicalism in the age of the American Revolution has only recently claimed the attention it deserves. Robert E. Toohey now makes a useful contribution, a welcome companion piece to Colin Bonwick's *English Radicals and the American Revolution* (1977). Concentrating on the brief but important period between the Coercive Acts and the Declaration of Independence, the author presents "essentially a history of ideas" (p. 25). The central preoccupation is the opposition to Lord North's American policy brought forward not by establishment critics like Chatham, Shelburne, and the Rockingham Whigs but by a small number of political thinkers termed "radical" by subsequent historians in the nineteenth century. Alarmed initially at the inroads upon constitutional liberty they conceived the British government to have made in the Wilkes affair of the 1760s, radicals saw in the American troubles confirmation of their deepest suspicions that a corrupt political system in Britain aimed to erect a new despotism throughout the empire. Appalled at the scene before them, they embarked upon highly individualistic and uncoordinated campaigns to arouse the British public to the danger. Convinced that the Americans who resisted the North ministry were defending liberty in Britain as well as in the colonies, an extraordinary band poured out a turbulent stream of pamphlets, books, and communications to the editors of London newspapers. Existing on the periphery of political life—dissenting influence was strong among them, a fact Toohey does not sufficiently appreciate—the radicals shared only two common denominators: the deep sense that something had gone desperately wrong with Britain's political system and the commitment to reforming this degeneration.

Five prominent radicals and their works are analyzed in some detail; and the author successfully achieves a prime objective: "to show that America did not possess a monopoly on men [*pace* Catharine Macaulay] with innovative, enlightened concepts of liberty and empire" (p. x). John Cartwright foresaw the Commonwealth. Granville Sharp anticipated responsible government. James Burgh foretold the Age of Reform. Richard Price gave liberty, the real issue of the American Revolution as he saw it, a broader applicability than even Locke and the eighteenth-century Whigs had done. In such mighty intellectual company, only the works of C. Macaulay, romantic and emotional propaganda, seem out of place.

Toohey is careful not to give radicals too much credit. A certain prescience notwithstanding, they did not create a new order at home or within the empire. Their immediate significance, indeed, arose

from "some influence" they exerted on American revolutionary thought. Regrettably, however, the nature and extent of this influence is not sufficiently explored. That American leaders read British radical literature, corresponded and entertained the authors, and expressed warm and friendly sentiments about them is clear. This is evidence merely of a certain coincidence of views, however, not of formative influences on the Americans. By comparison, Bonwick is much more successful in showing the impact of American revolutionary thought on some of these same radicals.

CHARLES R. RITCHESON
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ALBERT GOODWIN. *The Friends of Liberty: The English Democratic Movement in the Age of the French Revolution*. Cambridge: Harvard University Press. 1979. Pp. 594. \$20.00.

Friends of Liberty provides a more detailed account and subtler analysis of English reformers in the last decade of the eighteenth century than have hitherto been available. Others have surveyed longer periods of radical development, but none have used as much fresh material from English metropolitan and English and French provincial sources. Though at all times aware of antecedent and succeeding movements, Albert Goodwin has focused on the nature and growth of reforming political thought in the 1790s in London, Manchester, Sheffield (where he himself was born and educated), Norwich, and briefly in Ireland and Scotland. The different factors like Nonconformist disappointment at the failure to obtain relief from the penalties of the Test and Corporation Acts, continuing Commonwealth traditions, the stimulus of celebration of the revolution of 1688, the upheavals in France, and the eruption of Thomas Paine on the scene are all disentangled. From such a set of circumstances, added to an awakening consciousness of social and economic grievances by men like Thomas Spence, sprang radicalism of various degrees and sorts.

Goodwin describes a bewildering number of societies and associations; their functions are clarified and distinguished from earlier groups. Motivation and temporary cooperation with organizations in Ireland roused by events in France and in Scotland, adversely affected by wartime exigencies, are noticed, though only tangentially to English ideologies. London reacted to parliamentary conservatism and to polemics like Edmund Burke's *Reflections* (1791), as well as to its own small but persistent liberal element; Norwich responded to the commemoration of the Glorious Revolution; Manchester polarized the aspirations of rational dissenters and those of middle-class Whigs. In Sheffield arti-

sans were prompted by a depressed economy to form working-class groups, thereby setting a pattern for future urban organizations.

Agitation took various shapes; protests were systematized, old or new tracts were printed, and meetings were frequent. The government viewed all of this activity, even on the part of a small minority, as seditious. Arrests, often followed by trials, were made on the most fragile of pretexts. The court actions of 1794 are vividly described here. More valuable, if less dramatic, is the distinction spelt out between followers of Paine and other radicals. Before 1800 few were, like the author of *Common Sense*, genuine republicans. Beginning as proponents of religious liberty and a reformed franchise that should elect a Parliament less adverse to change, critics of other aspects of the establishment—the landholding system, the inequality of wealth and opportunity—became increasingly vocal, if not numerous, and eventually bequeathed fruitful traditions to nineteenth-century propagandists. Significantly, acts absolutely prohibiting all workers' combinations were passed in 1799 and 1800. Their repeal represented the first major if delayed victory for radicalism.

All students of English politics in the age of the French Revolution will find this splendid and original volume invaluable. Even the overlong, repetitious, and irrationally arranged bibliography should not deter either specialist or general reader.

CAROLINE ROBBINS

Rosemont, Pennsylvania

MICHAEL W. MCCAHILL. *Order and Equipoise: The Peerage and the House of Lords, 1783–1806*. (Royal Historical Society Studies in History Series, number 11.) London: The Society. 1978. Pp. x, 256. \$20.35.

Tocqueville liked to think of the English nobility as enlightened and intelligent. Michael W. McCahill shares that belief. He is not, to be sure, prepared to countenance all of the political foibles of the titled of the eighteenth century. He finds the peers bored with legislative responsibilities, inept at debate, and inordinately dependent for direction upon the newly minted brethren among them. But these disabilities, severe as they may seem, did not in McCahill's judgment prevent the nobility from playing a central and useful role in the political life of Great Britain: the peers were the great conservators or, to use McCahill's terms, the defenders of "order and equipoise."

McCahill's book is a study of the peerage in the age of the younger Pitt. Neither conceptually nor substantively does it break new ground. The author devotes chapters to such topics as the constitutional understanding of the peers; their service as legisla-

tors; their role in the great political shufflings of 1783–84, 1801, and 1804; and the nature of their political influence. His structural categories are as old as Namier: the peers of the realm are divided into king's friends, politicians, and independents. His major conclusions are equally unsurprising. Peers exercised wide political power by virtue of their landed wealth and their control of parliamentary boroughs. They were much like lobbyists in twentieth-century America, springing to action to protect specific, often local, interests. They justified these actions by reference to the need to defend the balanced constitution and the welfare of the community. Within the peerage the lawyers, the bishops, and the militia officers exercised preponderant influence. And whenever the peerage acted collectively as the House of Lords, it tended to submit to the authority of the few able men of affairs to be found in its ranks. In sum, McCahill simply confirms—though sometimes with arresting force—conclusions already widely accepted.

If the reader is disappointed at the tameness of these conclusions, she or he will be even more dismayed at some other aspects of the work. The opening suggestion that those historians are wrong who see the House of Lords as subservient to the crown is contradicted on almost every page: the author virtually admits as much when he tries to elevate the peers' unfailing readiness to support the monarch to the status of a disinterested principle. A similar discrepancy between conclusions and evidence arises when McCahill asserts that "the house of lords was not, during these years, the scourge of reformers" (p. 62). The reader's immediate doubts about that startling claim are not diminished when McCahill, seeking to justify it, cites as telling evidence such peripheral measures as regulations for hawkers and peddlers.

McCahill's work contains several useful appendices. For this reason, among others, students of the era will wish to consult it. But since the book neither justifies the Tocquevillian sensibility of its author nor alters the accepted view of the peerage in Pitt's day, the rewards will be fewer than readers would wish.

REED BROWNING
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GORDON C. BOND. *The Grand Expedition: The British Invasion of Holland in 1809*. Athens: University of Georgia Press. 1979. Pp. viii, 232. \$17.00.

This monograph deals with the Walcheren expedition of 1809. It is already well known that this operation was a fiasco. It was conceived as an effort to destroy French naval forces and dockyards in the Scheldt River and to relieve French military pres-

sure on Britain's Austrian ally. What was required for success was imaginative and bold leadership. Alas, instead of Wellington and Nelson there were only Chatham and Strachan. These officers share responsibility for failure with the government at home. Portland's ministry was singularly inept in devising a workable strategy; they also failed to realize that the expedition was headed toward disaster no sooner than it arrived off Zeeland. It collapsed amidst mutual recrimination between commanders, the dissolution of the government, and the ravages of diseases that incredibly incapacitated nearly half of some important divisions.

Gordon C. Bond has built his story around careful reading of both French and British sources. His strength lies in reconstructing the course of events. The book's merit is that it deals in great detail with all aspects of the invasion as it unfolded. The reader sees what each protagonist did. Though hardly revolutionary, this is useful work.

Yet there are questions that remain unanswered. Clearly, Bond is more familiar with French than British sources, an unfortunate circumstance given his focus. He fails to understand the conflict between Castlereagh and Canning or at least does not develop it fully in the context of the invasion. The chapter on the parliamentary inquiry into the failure is uninspired. It largely recapitulates the debate from public documents. What really transpired behind the scenes in light of ministerial changes? Does one explain the consistent government majority (in a full house by contemporary standards, a sure indication of the importance of the issue) solely on the merits of its case? Surely these are considerations of greater import than the movements of troops in Holland.

This, then, is traditional military and diplomatic history. London and Paris do things rather than British and French leaders. Broader questions such as the problems posed by combined military and naval operations are set aside. The result is an interesting but narrowly defined book of greater utility to the military specialist than to those who hope to find interpretations of larger issues. Some stylistic infelicities and curious references (such as calling the Treasury the "treasury office") contrast sharply with excellent maps.

CHARLES R. MIDDLETON
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PETER ALLEN. *The Cambridge Apostles: The Early Years*. New York: Cambridge University Press. 1978. Pp. x, 266. \$29.50.

Throughout the nineteenth century, student societies, especially debating clubs, served as an alter-

native system of education at Oxford and Cambridge. The rigidity of the formal curriculum, its exclusion of contemporary and controversial issues, and the university authorities' uncritical belief in the paramount value of examinations and competition created the vacuum that these clubs filled. Controlled by students, they tended to represent interests and values antithetical to the official system. Perhaps the most famous of these clubs was the Cambridge Conversazione Society, popularly known as the Apostles. The society is best known as the Cambridge origin of the Bloomsbury group, but its earlier history also included an impressive array of Cambridge intellectuals, from Frederick Denison Maurice, John Sterling, and Alfred Tennyson in the 1830s to Henry Sidgwick, Bertrand Russell, and G. E. Moore in the late nineteenth and early twentieth centuries.

Peter Allen argues in this new book that the society has been characterized by an enduring tradition of intellectual liberalism. The "Apostolic spirit" included a belief in unrestricted debate and in personal friendship unaffected by doctrinal differences, although it also contained a strong tendency to view with disdain all persons outside the clique. *The Cambridge Apostles* traces in detail these tendencies in the society from its inception in 1828 through the 1830s.

The real founder of the "Apostolic spirit" was F. D. Maurice, and Allen relates his abhorrence of rigid ideologies to the bizarre religious extravagances of his family, buffeted between a muddled Unitarian father and hysterical Calvinist sisters. In reaction, Maurice developed a Broad Church theology that emphasized the importance of unity and sympathy and rejected sectarian narrowness. Allen argues that the liberal spirit of Maurice's thought pervaded the society and that this spirit connected his "Apostolic theology" to Henry Sidgwick's faith in "social science" and to the estheticism of Bloomsbury.

The Cambridge Apostles raises a troubling question: how convincing is Allen's conception of an "Apostolic spirit" that has endured throughout the one-hundred-and-fifty-year history of the society? Liberalism means very little when it is stretched to cover so many different intellectual movements over so long a period of time. Often it seems that Allen is either straining to make connections or merely chronicling the disparate careers and activities of a group of college friends.

Nonetheless, it is clear that the society was the most important part of many Apostles' Cambridge education. It created the personal friendships and intellectual contacts that were to last a lifetime. The society was also of considerable practical value in furthering the careers of many Apostles by bringing promising young men to the notice of more estab-

lished intellectual figures. In *The Cambridge Apostles* we have a valuable case study in the concrete working of the social network that created what Noel Annan has called "the intellectual aristocracy of Victorian England." One may be skeptical of the existence of an enduring "Apostolic spirit," but one must recognize the importance of the society in connecting the university to the greater intellectual world. It would be interesting to know if other clubs (especially political ones) at Oxford and Cambridge performed analogous functions and how twentieth-century changes in the formal curriculum have affected these institutions of student self-education.

ARTHUR ENGEL

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PETER MARSH, editor. *The Conscience of the Victorian State*. Syracuse: Syracuse University Press. 1979. Pp. viii, 257. Cloth \$16.00, paper \$6.95.

"There is nothing in the whole world so unbecoming to a woman as a Nonconformist conscience," observes a character in Oscar Wilde's *Lady Windermere's Fan*. This volume suggests that not only Nonconformists and Gladstone but also other Victorians had a conscience and that, on balance, it was most becoming to them.

The contents of some of the essays, admittedly, throw doubt both upon that assumption and upon the title of the collection. As Joseph Hamburger observes in "The Whig Conscience," one finds in Thomas Babington Macaulay, the best-remembered Whig spokesman, a disdain for religiosity and "a fundamental rejection of public conscience" (p. 22). "Was the Nonconformist conscience rooted in a coherent view or philosophy of society and politics?" asks Richard Helmstadter in a related essay. By the 1890s, "clearly the answer must be negative" (p. 139). Yet, the leaders of British dissent had upheld a distinct ethos from the 1830s to the 1880s, a set of values based upon a theology of evangelicalism, a tradition favorable to liberty of conscience, and a record of significant if piecemeal political achievement. Paradoxically, at the very time that dissent became commonplace and that the dissenting bodies became most effectively organized, both their theology and their political *raison d'être* faded, and only the (for Helmstadter more trivial) dangers of wine, women, and gambling continued to rouse a united turn-of-the-century "Nonconformist conscience."

Helmstadter's is one of the two best essays in the volume. The other is David Roberts's "Utilitarian Conscience," a well-written, well-organized, and persuasive account of the parliamentarians, civil servants, and journalists who in the 1830s and 1840s sought to apply the "greatest happiness principle"

to politics. As Roberts demonstrates, that principle was at once potent, simple, and vague, and in practice the Utilitarians proved to be not Gradgrinds with an account-book mentality but men sufficiently concerned with "justice," with a hatred of tyranny, and with the possibilities of a reforming state to "impose on the Victorian ruling classes a most formidable public conscience" (p. 69). Roberts's essay is also well documented, in contrast to several others in which the documentation is curiously erratic.

The essay by John Cell is less concerned with "The Imperial Conscience" than with the manner in which the groups that, in the early nineteenth century had abolished slavery and the slave trade and had sought to impose the concept of trusteeship upon the government of India continued to influence relationships that, in an ultimate sense, rested upon military force and exploitation. Marsh's "Conservative Conscience" is taken up largely with Peel and Salisbury. For the latter, the defense of property and of the Established Church was "permeated with ethical considerations," and he "reinvested Conservative party loyalty with moral spine" (p. 232). Deryck Schreuder's "Gladstone and the Conscience of the State" is the longest and most ambitious but least successful of the essays, although the author illustrates the manner in which the statesman supplanted his youthful ideal of a confessional state with the vision of a public conscience that resided in "the Christian people of England at large" (p. 101).

Despite Marsh's illuminating introduction, some of the contributors find their subject elusive, and readers will remain unconvinced that "the Victorian state" had a conscience. They will be more readily persuaded that scores of Victorians indeed possessed consciences and that when they pondered the implications of public morality they were as likely to think of politics as of sex.

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KEITH ROBBINS. *John Bright*. Boston: Routledge and Kegan Paul. 1979. Pp. xvi, 288. \$21.50.

Keith Robbins has written a fair and informative biography within the word limit he or his publisher has set, but he does not explain fully all of the paradoxes in John Bright's career. An independent study based largely on original sources, it pays rather less attention to the secondary literature that has appeared in the field in the last few decades. No substantial biography of Bright has appeared since G. M. Trevelyan's official life was published in 1913. Some years ago Robbins wrote a new life of

Sir Edward Grey, a rather more scholarly study than the official biography also by G. M. Trevelyan. The present task is more difficult, however, for Trevelyan's life of Bright was much better than his *Grey of Fallodon*. Trevelyan may have been a little too partial to Bright, who was a friend and a parliamentary colleague of his father, but he made good use of the Bright papers and had great literary skill, regarding history, in his great uncle's tradition, as a branch of literature. Robbins's claims in the latter respect are more modest, and he is also at the disadvantage that Trevelyan used almost twice the space and gave his reader far more in the way of quotation from Bright's letters and speeches. There are, however, some areas in which Robbins has gone further, perhaps because his research was wider. Thus we learn more from Robbins about Bright's second marriage, the curious courtship, the long separations, the tendency for each partner to go his or her own way (although Bright, to the historian's advantage, was a most faithful correspondent). He was always protesting his loneliness in London but seemed to have had no compunction in traveling without Mrs. Bright on numerous fishing trips to Scotland, on some Continental tours, and in later life on visits to the homes of some of the great aristocrats whose order he had attacked so severely. Robbins also has a fuller account of Bright's breakdown in the 1850s and of the steps he took to recuperate. On the other hand, on most important topics Trevelyan's treatment is fuller and includes more detail.

There are two areas where one might have hoped a modern biographer to go beyond Trevelyan: first, Bright's business affairs and their relationship to his career and, second, Bright's role in the history of the Liberal Party and particularly of its radical wing. Neither aspect is ignored, but in each case one might have hoped for a fuller treatment. When Bright first entered Parliament it was clear that he regarded himself virtually as outside of party. Yet in 1875 Bright chaired the meeting that elected Hartington Liberal Party leader in the Commons and fully agreed with the decision. More attention might have been paid to the transformation that took place in the 1860s and also to Bright's relationship with other elements in the radical wing of the party. There are references (pp. 138, 164, 173) to Bright's concern for the party in the years 1858, 1862, and 1863, but they are quite incidental and bear development. Robbins might also have explored further the circumstances that led Bright to accept office from Gladstone in 1868, for there is evidence that Bright put pressure on Gladstone to change his mind on the ballot.

One might quibble about a few minor points. The India Act of 1854 (p. 98) did meet Bright's criticism of patronage as Trevelyan notes; Palmerston

introduced an India Bill in 1858, but it was never passed (p. 135); Gladstone won his Greenwich seat before he was defeated in Lancashire (p. 219). On the whole, however, the book is a well-researched and balanced but not unsympathetic interpretation of a unique Victorian, and we may agree with Robbins's final conclusion that whatever Bright was he was undoubtedly "his own man."

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University of Toronto

NORRIS POPE. *Dickens and Charity*. New York: Columbia University Press. 1978. Pp. xi, 303. \$16.50.

Norris Pope's title misleads. Those seeking further information about Dickens's attitudes toward charity as *caritas*, and those wanting details of Dickens's own charities or of his close association with leading philanthropists like Angela Burdett Coutts on Urania Cottage and in Columbia Square, should consult Dickens's letters and speeches, the Coutts archives, the work of Humphry House and K. J. Fielding, and Edna Healey's recent biography of the baroness. Nor will one find in Pope's monograph an examination of the role of charity in Dickens's philosophy or fiction: there is no extended discussion, for instance, of the satire on the poor laws in *Oliver Twist* or of the complex vision of private and public responsibility fundamental to *Bleak House* and *Little Dorrit*.

Instead, Pope's "charity" is Evangelical philanthropy, and his focus is on Dickens's ambivalent response to Exeter Hall. Pope divides the topic into five sections: an overview of Dickens's quarrel with narrow, presumptuous religionists; Dickens's lifelong opposition to sabbatarianism; his attacks (especially in *Bleak House*) on the misguided efforts of missionary societies; his qualified endorsement of the evangelically dominated ragged school movement; and his useful if limited propaganda for sanitary reform and improved housing. Each chapter is densely packed with information about prominent evangelicals, their reformist objectives and strategies, and the institutions they founded, such as the Lord's Day Observance Society, the Ragged School Union (RSU), and the Society for Improving the Condition of the Labouring Classes. Juxtaposed to the flourishing and waning of these bodies are Dickens's public speeches and *Household Words* articles (particularly those denouncing Lord Ashely's Sunday Post Office Address and supporting the Board of Health and its public relations agent, the Metropolitan Sanitary Association), his letters to influential friends like Coutts, Macvey Napier, Southwood Smith, and his brother-in-law Henry Austin, and key passages from a few novels.

Pope's extensive research into the archives of the

RSU and the pages of the leading Low Church journal, the *Record*, yields dozens of helpful quotations about these mid-Victorian voluntary efforts to ameliorate specific social evils and illustrates both the hopeful beginnings and the mixed consequences of attempts to deal symptomatically with urban poverty, ignorance, vice, overcrowding, and disease. Dickens's ardent concern for the poor often led him to support these evangelical causes while steering clear of narrow sectarian emphases. For example, he frequently praised ragged schools like the one in Field Lane, even after Mayhew's sharp attack in 1850 on the RSU in Dickens's old paper, the *Morning Chronicle*. Dickens differed from ragged school activists in deploring the time wasted in classrooms on dead-end theological controversies or flopping in prayer, and he always thought of philanthropic education projects as transitional until comprehensive national programs could be enacted. On the ragged school movement, as on health and housing, Dickens's involvement was extensive, discriminating, and developmental.

The later chapters of this book contain more of interest than the former (where Dickens's attitude is predominantly critical and satiric) and better integrate historical narrative with Dickens's response. An important question raised by this study is the extent of Dickens's influence on mid-Victorian social reforms. *Dickens and Charity* does not attempt to settle the issue, but it opens and closes with quotations that define the poles of Victorian opinion about it. The first, from a nonconformist preacher to his congregation, asserts that "there have been at work among us three great social agencies: the London City Mission; the novels of Mr. Dickens; the cholera." The last, from Shaftesbury's diary, affirms public awareness of Dickens's social conscience: "God gave him . . . a general retainer against all suffering and oppression." But the old veteran, battle-scarred, went on to confess sadly, "And yet, strange to say, he never gave me a helping hand—at least, I never heard of it."

ROBERT L. PATTEN
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RAYMOND G. COWHERD. *Political Economists and the English Poor Laws: A Historical Study of the Influence of Classical Economics on the Formation of Social Welfare Policy*. Athens: Ohio University Press. 1977. Pp. xvii, 300. \$15.00.

No one can complain that the English poor laws have been neglected by historians. Scholarly studies of the theory and practice of poor relief continue to proliferate, making ever more daunting the task of a new synthesis to replace that of the Webbs. Ray-

mond G. Cowherd's study is concerned with the intellectual background of the New Poor Law of 1834, and covers much the same ground as J. R. Poynter's *Society and Pauperism* (1969). Comparisons are thus inevitable, with results that are clearly in favor of Poynter. Cowherd's book is marred by structural and interpretive defects that render it less useful to the specialist and sometimes misleading to the unwary general reader.

There is much valuable biographical information here on both the major and minor figures of eighteenth- and early nineteenth-century Britain who grappled with the thorny problems of poor-law reform. Indeed, the title of the book does not adequately convey the range of thinkers covered. Not only are classical economists like Malthus, Ricardo, and McCulloch discussed but also a number of evangelicals, utilitarians, and political leaders. A central problem, however, is that the author has a passion for attaching labels to virtually every figure he discusses. The generation or so before 1834 is seen as a great debate among humanitarians (led by evangelicals), natural-law reformers, and philosophical radicals. An attempt to adhere too rigidly to this classification obscures the real complexity of the debate in which many thinkers, as well as the public, combined influences from all three groups as well as from other value systems and schools of thought.

The same problem is evident in the author's attempt to label the reforms of the period. Thus Gilbert's Act of 1782 and Pitt's reforms of the 1790s are described as humanitarian, Sturges Bourne's Act of 1819 is said to proceed from the natural-law reformers, and the New Poor Law is treated as a piece of applied philosophical radicalism. Such a classification ignores many elements of continuity linking those statutes, like the grouping of parishes and the shifting of power from the smaller ratepayers to the major tenants and landowners. There was undeniably a growing spirit of deterrent rigor in the nineteenth century, cumulating in the workhouse test and the principle of less eligibility, but the author exaggerates the shift. This is compounded by a factual error (p. 276)—that the New Poor Law stopped all outdoor relief effective June 1, 1835, when actually this provision was deleted by the House of Lords. From the author's account of the 1834 statute, a general reader would surmise that only in dire emergencies did the able-bodied poor receive outdoor relief—a gross distortion, as anyone familiar with the recent literature on the act's implementation can attest.

There are, indeed, a number of significant omissions from the author's bibliography, most notably on the subject of the "revolution in government." If Cowherd had taken into account other scholars' contributions to this subject, he might have avoided

the rigid categories and distortions that detract from an otherwise useful book,

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ANNE DZAMBA SESSA. *Richard Wagner and the English*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 191. \$13.50.

In the closing pages of *Richard Wagner and the English*, Anne Dzamba Sessa concludes, "The fact is that Wagner was a cultural phenomenon; his music dramas seemed to offer philosophic and psychic inspiration to late Victorians in a changing and challenging era." The pages that precede this conclusion attempt to illustrate the kinds of inspiration that Wagner's music dramas offered to the British between the 1850s and 1914 and the kinds of admirers, of widely varying talents, who were duly inspired. Bernard Shaw gets a chapter to himself, as does an obscure and pretentious pseudo-philosopher named David Henderson Irvine. The "literati" are squeezed together in another chapter, so that Swinburne, Beardsley, George Moore, and Arthur Symonds are tossed in among the likes of John Payne, John Davidson, and Theodore Wratislaw, an English decadent poet of Polish origin. Finally, there is a chapter on the English response to *Parsifal* in light of the late-nineteenth-century search for surrogate religions to replace the Christianity undermined by science and Biblical scholarship.

Along the way there are some valuable observations. The author rightly emphasizes that Wagner represented different things to different people—prophet of socialism to Shaw, champion of sensualism and eroticism to Beardsley, critic of the conservative establishment to Irvine, and, to some Theosophists, chronicler of man's evolution toward perfection. Sessa explains clearly the importance of myth in Wagner's work and makes a persuasive case that Wagnerism must be included among the numerous forms of escape from scientific materialism that flourished in the late nineteenth century.

These nuggets, however, are unfortunately wrapped in an unprepossessing package. The author's favorite stylistic device is the list, which she employs at every opportunity. There are lists of authors who cite Wagner in their writings, lists of Wagner citations in the works of individual writers, and a lengthy list of the contents of the *Meister*, a short-lived English journal devoted to the study of Wagner. All too frequently, lists replace genuine analysis; an author's indebtedness to Wagner is "explained" merely by listing all references to Wagner in that author's work. And when analysis is ventured, the author tends to rely heavily on clichés of

late Victorian intellectual history. We are told, for example, that the age was "increasingly uncertain and dissatisfied with itself" (p. 49) and that it suffered from a "spiritual vacuum" (p. 134), but scarcely any effort is made to put fresh meaning into these tired phrases.

JANET O. MINIHAN
American University

HAROLD ISSADORE SHARLIN. *Lord Kelvin: The Dynamic Victorian*. In collaboration with TIBBY SHARLIN. University Park: Pennsylvania State University Press. 1979. Pp. xiv, 272. \$16.00.

William Thomson, Baron Kelvin, was a seminal figure in Victorian science. His six hundred and sixty-one published articles (at least two annually from 1841 through 1907) touched on nearly every significant physical problem of the age and much else besides. His skill at applying abstract mathematical physics to practical technology made trans-oceanic telegraphy possible and greatly improved marine navigation. His *Treatise on Natural Philosophy*, written with P. G. Tait, transformed the teaching of physics in Britain and much of the rest of the world. But more important than any of these, his personality and the extraordinary fertility of his ideas provided inspiration for three generations of physicists. As an eminent younger contemporary remarked, an account of nineteenth-century physics that failed to deal with Kelvin "would be like the play of *Hamlet* without the Prince of Denmark."

Kelvin's reputation, unparalleled in his own time, has suffered a severe eclipse in ours. He contributed significantly to many fields of science but produced no lasting synthesis in any. His contributions to electricity and magnetism were overshadowed by those of Faraday and Maxwell, his work on thermodynamics by that of Clausius and Joule. And he has never been forgiven his "wrong-headed" opposition to the theories of Maxwell and Darwin. Rather than the Prince of Denmark, he has been relegated to the role of Polonius—necessary for the plot, sometimes quaint, but ultimately an obstacle to the central characters.

Harold Issadore Sharlin's admirable aim is to change this state of affairs. "I have tried," he says, "to integrate the subject's life and work and to place the result in a broader historical context that would illuminate the history of science and technology in the nineteenth century" (p. ix). Unfortunately the effort fails. Readers of this book will find little to explain Kelvin's reputation among his contemporaries and still less to clarify his position within the larger framework of Victorian science. By basing his account almost exclusively upon Kelvin's own correspondence and published works, Sharlin has put

blindness on his perspective. The world outside of Kelvin's immediate circle is ignored and only the most obvious of his interactions with other scientists are examined. No mention is made of the scientific organizations that played such a large part in Kelvin's life and in which he played so important a role. In the few instances where Sharlin attempts to deal with Kelvin's contemporaries, the selection is erratic and is rarely focused sharply on the issues at hand.

Even when dealing with Kelvin himself, Sharlin paints a curiously unbalanced picture. A third of the book is devoted to Kelvin's life before he became professor at Glasgow at the age of 22. (He lived to be 83.) More space is devoted to the political wrangling of Kelvin's father in securing the Glasgow chair for his son than to Kelvin's seminal work on thermodynamics or his revealing objections to Maxwell's electromagnetic theory. The correspondence with Tait over the writing of their textbook is quoted in detail while years of intense professional activity are skipped over with hardly a notice. The treatment of Kelvin's mature life is fragmented and two dimensional—an occasional vignette inserted in a stream-of-consciousness organization—while the heart of the book, the analysis of his scientific work, is often obscure and confusing.

To Sharlin's credit, he has tried to avoid hero-worshipping his subject. He is at his best in narrative accounts such as his treatment of Kelvin's activity on the Atlantic cable project and in dealing with integrated bodies of correspondence such as that between Kelvin and G. G. Stokes. But these virtues cannot save this disappointing book. When we understand Kelvin's role in nineteenth-century physics, we will have greatly enlarged our knowledge of Victorian science, but this book contributes little toward that end.

JOE D. BURCHFIELD
Northern Illinois University

ROBERT STEVENS. *Law and Politics: The House of Lords as a Judicial Body, 1800–1976*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1978. Pp. xviii, 701. \$30.00.

This is a history of the House of Lords extending over almost two centuries and centering on the judicial function of that house. Robert Stevens has taken a broad view of his subject, considering not only the House of Lords as the final court of appeal in the United Kingdom but also the ways in which the development of the law was shaped by the political and social outlook of the law lords. The outcome is a substantial volume that tells much about the law and the law lords, but less than one would

like to know about the relationship between their views of their judicial function and the major turning points in the history of the House of Lords. That is, *Law and Politics* is more successful when Stevens writes about the law than when political history pre-empted the stage. Unmistakably, however, this is a major piece of work, representing a vast expenditure of time and effort, and Stevens has discussed in considerable depth an area of legal history that was until now unmapped and uncharted.

The main theme is that the House of Lords—after it became clear in 1844 that the law lords alone could vote in judicial matters—gradually ceased to be a political body and became increasingly “scientific” in exercising its judicial function. The more political phase, which culminated in the Parliament Act of 1911, was in the late nineteenth and early twentieth centuries when Salisbury, Balfour, and Bonar Law led the Conservative Party and Lord Chancellor Halsbury guided the law lords. It came to an end with the advent of the Liberal Lord Chancellors Herschell and Loreburn, who for their own party reasons started the House of Lords on the path to what is called “substantive formalism.” The term means that the law lords acted as if the courts should be thoroughly subordinate to the sovereign legislature. Lord Chancellor Haldane carried the trend further in the name of scientific objectivity, and thereafter judicial restraint was increasingly the order of the day. Only in the last twenty years have the law lords proved receptive to the idea that judicial decisions constitute a legitimate form of law-making. Two law lords, according to Stevens, were of special consequence in this connection. A major effort was made to arrest the decline of the House of Lords as a judicial body during the chancellorship of Lord Kilmuir (1954–62); and, in the subsequent reversal of substantive formalism, Lord Denning played a key role when he “trumpeted the lawmaking potential of the judiciary from the rooftops” (p. 490).

An obstacle to this kind of study rises out of the dearth of scholarly monographs that would permit greater precision in interpreting the history of the House of Lords. As a result, Stevens tends to rely excessively on the histories of the period associated with R. C. K. Ensor, Trevor O. Lloyd, and Arthur Marwick—works too general for his purposes. To be sure, he has undertaken his own research at certain points, as in an interesting description of the retention of the lords' appellate jurisdiction in 1876. Yet there are places where more information would be helpful. For example, the statement that the mixed constitution was effectively at an end by 1830 requires more elaborate explanation than is given here. After all, Olive Anderson, writing in the *English Historical Review* (1967) about the Wensleydale peerage case of 1856, attributes the failure to in-

roduce life peerages at this time in large measure to the great popularity of the theory of the mixed constitution. The proposed creation by Queen Victoria was opposed as a threat to the vaunted balance among king, lords, and commons. Under these circumstances Stevens's language is hardly a preparation for Anderson's description, and the discrepancy should be ironed out. But these are relatively minor matters in a work of this scope. As earlier noted, *Law and Politics* is a welcome addition to modern histories of the House of Lords, especially for those who believe that that house merits a larger place in the literature of parliamentary institutions than it has hitherto received.

CORINNE COMSTOCK WESTON
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ARTHUR DAVEY. *The British Pro-Boers, 1877-1902*. Cape Town: Tafelberg. 1978. Pp. 220. R 13.00.

Arthur Davey states that his purpose is "to give a comprehensive history of the British pro-Boers, not only in the major phase of 1899-1902 but also in the preceding quarter century during which the phenomenon evolved." He defines the British pro-Boers as "those citizens of the United Kingdom who by their utterances, writings or other actions showed recognizable signs of sympathy for or understanding of the South African Dutch or Boers" (p. 2). His preface carefully relates the work to other scholarship. There is a very full bibliography, a note on sources, and an excellent index. The extensive footnoting is conveniently at the bottom of the page. Dutch quotations in the text are translated in the footnotes. South African archival materials were thoroughly exploited and relevant private papers in Britain and in South Africa, as well as the most useful newspapers and periodical sources, were consulted. For someone interested in the Boer War period, the book, therefore, has interest and value.

Workmanlike the study certainly is, but there are limitations imposed by its task. The obligation to be comprehensive leads to an almost encyclopedic style. Fascinated as he is by the activities per se of the pro-Boers, Davey eschews theory and does not really analyze ideas. The excitement of debate is absent from the book. What is new and interesting, consequently, is to be found in the richness of detail and not in the interpretation. The author assumes moreover, that his readers have considerable knowledge of the Boer War and British history. The general reader is told too little to comprehend many of the events animating the pro-Boers, which are referred to but not explained (for example, the Jameson Raid and inquiry, prewar diplomacy, even the conduct of the war itself).

Davey does draw comparisons between 1877-81, when retrocession did occur, and 1899-1902. Missing in 1899-1902 was a figure of Gladstone's stature to restrain rampant jingoism. Anti-Boer propaganda was skillful; the Boer ultimatum and early invasions and annexations of British territory were hard to excuse. Some pro-Boer activity was extreme and counterproductive. The pro-Boers thus failed to moderate British prewar diplomacy or to prevent the annexation of the Boer republics. They had little effect on the course of the war, and only after the burning of farms, the concentration camps, and the frustrating prolongation of the war had worked on public opinion could Henry Campbell-Bannerman and others wear down the policy of unconditional surrender. The pro-Boers may, nevertheless, have underlined for Britain the anti-imperialist lessons of the war, and their concepts of conciliation and their faith in self-government were ultimately applied by Campbell-Bannerman when he became prime minister in 1906.

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GREGORY D. PHILLIPS. *The Diehards: Aristocratic Society and Politics in Edwardian England*. (Harvard Historical Studies, number 96.) Cambridge: Harvard University Press. 1979. Pp. viii, 288. \$18.50.

Until recently the British right in the Edwardian period has, with the exception of Bernard Semmel's pioneering work, been sadly neglected or shallowly dismissed. Controversies about the emerging labor movement and the state of liberalism and the Liberal party have absorbed the energies of most historians. Of late, works by Springhall, Ramsden, Searle, and Steiner have, in various ways, perceptively treated aspects of the right on both the ideological and the active political levels. We are treating more seriously than before the influence of such organizations as the National Service League and the Boy Scouts as well as examining in detail the Germanophobe press. Gregory D. Phillips's book on the Diehards is a valuable, meticulous addition to this growing if still sparse literature.

Phillips is the arch revisionist, attempting to render respectable the most maligned group of aristocrats in modern British history—the so-called "Backwoodsmen" who voted against the 1911 Parliament Bill and whose rhetoric added to the "sense of impending clash" that pervaded Britain in the period 1911-14. By arguing that the Diehards were hardworking, serious politicians acting from considered intent and not "instinctive reaction," Phillips goes against most contemporary accounts as well as the interpretations of historians such as George Dangerfield and Roy Jenkins.

The most valuable and original parts of the book are those chapters that marshal data and arguments to show that the 112 Diehards were, on the whole, active politicians who better than most of their class had adapted successfully to the economic and political challenges of the late Victorian period. Phillips argues convincingly that their intransigent behavior in 1911 cannot be attributed to frustration at the local level, where they still held sway, and must be attributed to their attitudes and activities on national and imperial affairs. In these areas, the Diehards differed from fellow aristocrats primarily "in their persistence, the strength of their commitment and the leading role which many of them took in military and imperial controversies." Even here Phillips claims that the Diehards were selective and not absolute in their resistance to change.

When describing Diehard ideological beliefs, opinions, and prejudices, Phillips is less convincing. The litany of their faith in tariff reform, conscription and "efficiency" and of their abhorrence of democracy, socialism, and liberalism does not supply the much needed reflective work on conservative thought in prewar Britain. Were not the Diehards responding to immediate issues arising from the Liberal victory of 1906 rather than a school of thought? Phillips attempts to give ideological coherence to the Diehards by classifying them under the term "Radical Right," which Lipset and Bell in the 1950s tried to apply to groups at the extreme right in the American political spectrum. Milner and a few other Diehards might qualify for the label "Radical conservatives rather than simple reactionaries" but most of the 112 certainly do not. Moreover, leaders such as Milner and Selborne were Liberal Unionists and not Conservatives.

On a methodological level problems arise from Phillips's concentration on the 112 peers while neglecting their Diehard comrades in the Commons. He might also have noted how effectively the industrialist Bonar Law tamed the Diehards where the aristocrat Balfour failed. Finally, while acknowledging Diehard willingness to accede to civil war before 1914, particularly over Ireland, Phillips underplays the destructive impact they had on British political life by 1914 in his eagerness to convince us that the Diehards were adaptable and farsighted on some issues.

RICHARD A. REMPEL
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JOE GARNER. *The Commonwealth Office, 1925-68*. Exeter, N. H.: Heinemann. 1978. Pp. xix, 474. \$60.00.

Joe Garner joined the Dominions Office in 1930—five years after Leo Amery had created it in an attempt to persuade South Africans, Irishmen, and others that, as they were no longer administered like "subject peoples" under the Colonial Office, they

were indeed fully autonomous nations freely associated in the British Commonwealth. Garner retired from the Commonwealth Relations Office in 1968 on the eve of its merger with the Foreign Office. He is therefore in an unusual position from which to write an insider's "house history."

Compared with the official histories of British government departments that come to mind—Hall, Parkinson, and Jeffries on the Colonial Office, for example—Garner's book is a large and ambitious undertaking indeed. Here to be sure are the in-house anecdotes, the brief portraits of ministers and fellow colleagues, the discussion of office routine that one expects from such a work. Garner, however, has taken seriously the task of setting his office in the context of what it was doing: to try to keep the Commonwealth together through delicate, painstaking, and necessarily colorless diplomacy with a group of extremely sensitive and aggressive junior partners. Up to the termination data set by the thirty-year rule, he has explored in some detail the documents on such central problems as Ireland, the abdication crisis, imperial preference, wartime cooperation, and the transformation of the white man's club after World War II. This material has been covered thoroughly and well by Mansergh and others; quite rightly Garner frequently cites secondary as well as primary sources; and there are few surprises.

Of greater interest is Garner's inside perspective on more recent topics such as Suez, relations with the independent African countries, and Rhodesia. Here again he mainly confirms the contemporary official statements: he agrees with the late Iain Macleod that there was no planned acceleration of the march toward independence after the election of 1959; he argues that the very mistaken belief that Ian Smith was a "man of straw" who would be toppled "within weeks" by a bit of economic pressure was based on the assessments of Sir Roy Welensky and other Rhodesian political veterans. For some reason Garner chose not to exercise the privilege that is often accorded to retired officials of using documents not yet available to outsiders, a decision for which professional historians should be grateful.

Though hideously expensive, this book is indispensable for research libraries if not for individual purchase. It was high time for a house history of the Dominions Office, and Garner has conceived and written it well.

JOHN W. CELL
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ASA BRIGGS. *The History of Broadcasting in the United Kingdom*. Volume 4, *Sound and Vision*. New York: Oxford University Press. 1979. Pp. xiv, 1,082. \$65.00.

Asa Briggs's latest volume in *The History of Broadcasting in the United Kingdom* deals with a crucial mo-

ment in the development of the mass media in Britain: the advent of commercial television. Parliament's decision to allow the BBC's monopoly to be broken by the passage of the Television Act of 1954, which set up a public authority with powers to allocate franchises and oversee program quality, came at a moment when, as he makes clear, the BBC was still coming to terms with television. In fact, the men who ran BBC television were engaged in a frustrating and often petty struggle with a central bureaucracy that thought of television as an adjunct to radio: "radio with pictures," as one BBC mandarin put it.

One consequence of this attitude was the resignation of the head of BBC television, who proceeded to become one of the moving spirits of the commercial television lobby. The story of that lobby and of the early days of commercial television inevitably takes up much of the present volume. Commercial television, as Briggs makes clear, was an issue that divided the two main political parties internally. Thus there was strong opposition to commercial television within the Conservative Party because it ran counter to the patrician tradition of public service; within the Labour Party there were advocates of commercialism who felt that the BBC was too firmly establishmentarian.

Briggs's account, in spite of interviews with prominent participants of the events he describes, indicates one of the problems that will face future volumes in the series. Although the BBC and the Independent Broadcasting Authority, which now controls both commercial radio and commercial television, have put their archives at his disposal, there is no such easy access to the papers of the commercial companies that have been involved in a venture that, after a shaky start, became in the words of Lord Thomson (later proprietor of the *Times*) "a licence to print money."

In spite of a noticeable reluctance to make a posteriori judgments, Briggs has provided a well-balanced account of a historical episode, which throws light not only on the nature of the BBC and British commercial television but also on the workings of the British political machine. It is in that sense a study of how the role and functions of the mass media are controlled within a parliamentary democracy.

STUART HOOD
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P. W. J. RILEY. *King William and the Scottish Politicians*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1979. Pp. xi, 194. \$27.75.

This is a fascinating and in some ways provoking book. It elaborates on views P. W. J. Riley has ex-

pressed before (see his "The Structure of Scottish Politics and the Union of 1707" in T. I. Rae, ed., *The Union of 1707: Its Impact on Scotland* [1974]) and provides the first detailed account of the machinations of Scottish politicians during the reign of William II and III, "a study of political chicanery at the highest levels . . . not calculated to lighten anyone's spirit" (p. 10). It is also a study in failure at all levels; no one emerges with any credit save possibly the first marquess of Tweeddale, who, unlike his fellow politicians, "concerned himself seriously about the wellbeing of Scotland" (p. 50) and the earl of Seafield, a far-sighted man who had something of the outlook of a permanent civil servant. The king cared nothing for Scotland; all he wanted was that it should cause him no trouble. He left its management to his favorite Portland and the returned exile William Carstairs. According to Riley, they all misunderstood the nature of the Scottish problem. They imagined that issues were involved, specifically a religious issue between presbyterians and episcopalians, when in fact the only thing that mattered was office. William understood enough about the tactical requirements of Scottish politics to resist the destruction of the committee of the articles. What he failed to understand was that once the committee was gone, and the independence of the Scottish parliament assured, that body would become unmanageable because there would not be enough places and pensions available to create a working majority. Nor did William and his advisers properly assess the place of the magnates in Scottish political life. There were four such men, so powerful that no government could survive without the support of at least one of them: the dukes of Hamilton and Queensberry, the marquess of Athol, and the earl of Argyll. If it was impossible to govern without them, it was also impossible to govern with them, because none of them would share power with any of the others, much less with any less important man. After William's early attempts to govern without them failed, a ministerial reconstruction in the winter of 1695-96 resulted in a government in which all four interests were represented. The result was an alliance between Queensberry and Argyll that drove the other two out; this accomplished, Queensberry began to intrigue, successfully, against Argyll. At the end of the reign Queensberry stood alone, facing a parliament that his insatiable greed for dominance had rendered unmanageable. The only solution, as William came to realize (though probably, says Riley, for the wrong reasons), was union.

Riley tells his sordid story artfully; his research is thorough and his conclusions convincing—though for reasons that are not wrong but incomplete. He has become so immersed in his tale of political chicanery that he deals with events only as they affected the struggle for office, and he sees them too

much in that light. Of the massacre of Glencoe, for instance, he writes, "the outcry was more against Dalrymple than the crime" (p. 84). This outlook leads him unduly to discount the impact of public issues on the politicians themselves. William may have exaggerated the importance of the religious issue, but the nature of the religious settlement did matter to people, and it is worth pointing out that each of the shifting coalitions of officeholders operated in the presbyterian "interest." If Riley's approach had been a little less cynical than that of the Queensberrys and Dalrymples, this would have been a better book. But it is a very good one, an important contribution to our understanding of the antecedents of the Anglo-Scottish union.

MAURICE LEE, JR.
Rutgers University

MALCOLM GRAY. *The Fishing Industries of Scotland, 1790–1914: A Study in Regional Adaptation*. (Aberdeen University Studies Series, number 155.) New York: Oxford University Press, for the University of Aberdeen. 1978. Pp. 230. \$19.50.

The importance of fishing as an additional support for the Highland economy during the years of severe population pressure has long been recognized. Despite this no specialized study of fishing has ever been written nor has the role of fishing elsewhere in Scotland been investigated. Malcolm Gray's book thus fills a gap in the literature on Scottish economic history. The first five chapters describe and explain the remarkable transformation that lifted the Scottish fisheries from their eighteenth-century backwardness to a position of preeminence within Europe by the mid-nineteenth century. Public policy sought to develop the fisheries, but Gray allows this little weight in his explanation of the dramatic change in the industry's circumstances. Rather, it was "tiny private enterprises" (p. 6) that tapped the abundant, readily accessible stocks of herring, haddock, cod, and ling, in response to demand from three main markets: Ireland, the West Indies, and Continental Europe.

The book is described as being primarily an economic analysis, but in fact there is plenty to interest the social historian in Gray's analysis of the fishing communities. Three broad geographical regions, each with its own distinctive social organization, are identified: the East Coast (with Caithness as the pioneering area in the development of herring fishing), the West Coast (with the Firth of Clyde treated separately because of the preponderant influence of the Glasgow market), and the Northern Isles. In the East, fishing became a fully professional, year-round activity divorced from farming. On the West Coast, land hunger, poverty, and lack

of landlord support left fishing as a part-time activity providing some extra income and subsistence. In the Northern Isles, land provided a less than full subsistence, but, unlike on the West Coast, landlords were active in promoting and profiting from fishing. The continuing influence of these characteristics is traced in later chapters and is fundamental to Gray's view of fishing as a series of industries rather than a single industry. This may not accord with the economists' definition of an industry, but it undoubtedly enables Gray to make fascinating observations about the fishing communities. Thus, for example, the custom of equal shares, common on the East Coast in the 1790s, created a community that "was almost class-less within itself" (p. 24). Such equality was not present elsewhere nor did it survive the growing intensification of the industry as steam-trawling (from the 1880s) and the steam-powered drifter (after 1900) radically altered the conditions of participation in the industry. This theme and the part played in it by the fish salesmen, a small but wealthy and influential group who took over marketing from the curers in the 1880s, dominate the rest of the book. With their multiplicity of shore-based business interests, the salesmen provided market services that made Aberdeen the most rapidly growing city in Scotland between 1880 and 1911. Not all of the expansion in fishing capacity before 1914 proved financially rewarding, and with a heavy dependence on export markets (some 80 percent of the herring catch in 1913 went to Europe) the industry was to suffer severely during the interwar period.

RON WEIR
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CORNELIUS O'LEARY. *Irish Elections, 1918–77: Parties, Voters, and Proportional Representation*. New York: St. Martin's Press. 1979. Pp. 134. \$19.95.

Irish politics is proving increasingly fruitful for testing concepts of comparative politics. As theories about nation-building, elites, leadership, and political parties have been used in recent studies of Irish political culture, psephologist Cornelius O'Leary is just the person to make an appraisal of its system of proportional representation (PR). Well versed in the Anglo-American experience with single-membered constituencies where the winner takes all and with the theory and practice of PR on the Continent and in Ireland (having written *The Irish Republic and Its Experiment with Proportional Representation* in the Notre Dame series of national election studies), he should be able to test opinion and theory about Irish polling against experience and reality.

O'Leary shows that Ireland's system of proportional representation has performed contrary to Anglo-American opinion about elections. Ireland has a

system of multimembered constituencies where electors can exercise the single transferable vote, thanks to Lloyd George's efforts to preserve minority representation during the fight for independence. Irish elections have not worked against responsible, representative government despite the Republic's social and political differences. In fact, the originally diverse and fragmented group of parties was reduced to a system of two-and-a-half parties by the 1960s, a process that effected stable government. In achieving stability, parties, leaders, the media, and voters all played a role. Although O'Leary admits that there is a complex, interdependent relation between parties and electoral systems and that the Irish experience has undercut some of the dogmatism about majority systems, he does not believe that the Irish system *per se* has contributed much to government stability.

The trouble with O'Leary's explanation is that it does not give Irish PR its due, thanks apparently to his past dogmatism about majority systems. Despite his claim, the first half of the present volume is little more than a rehash of the earlier work, warts and all. For example, in the midst of describing how Irish elections helped achieve a stable party system, O'Leary asserts, "Apart from all value judgments PR has, in fact, acted as a disintegrating force and the majority system as an integrating one" (p. 50). PR's supporters, like J. A. Costello, are pictured as little better than fools and knaves although they appear to have been more right about the future and about what might happen if PR were eliminated than people like David Thornley. The roots of O'Leary's dogmatism seem to lie in the assumption that elections are meant only to create stable governments and in a bias toward Ulster and Unionism. Electoral systems, like parties and other institutions, have many functions, including mobilizing and educating citizens so that they can trust their leaders and their policies. O'Leary should have explained these other functions of Irish elections, including local ones, against a backdrop of the horror that the majority system has helped create in Northern Ireland. The Stormont regime had such overwhelming majorities that it lost the confidence of friend and foe alike.

The Irish tragedy is too complex and confounding for O'Leary's electoral preferences.

TROWBRIDGE H. FORD
Holy Cross College

BRIAN P. COPENHAVER. *Symphorien Champier and the Reception of the Occultist Tradition in Renaissance France*. New York: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. 368. DM 92.

The "enchantment" and "disenchantment" of the Western world is a topic that has increasingly con-

cerned both intellectual and social historians. D. P. Walker and Frances Yates have shown that, although occultism existed in antiquity, in early Christian times, and throughout the Middle Ages, it took on greater respectability and force during the Renaissance, largely because of the influence of Marsilio Ficino and Giovanni Pico della Mirandola.

Brian P. Copenhaver has contributed a worthy study to the growing corpus dealing with the rise and decline of occultism, focusing his considerable scholarly talents on Symphorien Champier, the Lyonnais humanist-physician of the early sixteenth century. Duke Antoine of Lorraine's *primarius medicus* was, according to Copenhaver, a scholar of "un-deviating unoriginality." Yet his very limitations make him a valuable object of study, for Champier reveals the mental space of similarly educated intellectuals of his day.

Eschewing the social scientific approaches to the study of Renaissance occultism found in the works of Keith Thomas and Erik Midelfort, Copenhaver investigates the "intellectual history of the occultist tradition through the examination of individual thinkers." This examination allows Copenhaver to answer the question posed in his "introductory essay": "to what degree and for what reasons did the occultist tradition possess 'cognitive authority' [i.e., intellectual respectability] in the late medieval and early modern period?"

It would at first seem strange that Copenhaver has studied Champier in order to answer this question. After all, Champier was well known as an enemy of occultism, especially of the astrologers and of the platonizing medieval Arabic physicians. (Champier's *Dyalogus . . . in magicarum artium destructionem*, appended to this study, is an example of Champier's attack on the occultist tradition.) An examination of Champier's writings and of his sources demonstrates, however, why he transmitted the occultist tradition at the same time that he was its critic and enemy. Champier feared and criticized magic in its various forms because of its tendency to negate human free will and God's providence. Yet this medical humanist's acceptance of the *prisca theologia*, the Greco-Roman and Islamic medical authorities, and the medical advice of Plato's *Charmides* (156d-157a), as interpreted by Ficino, necessitated his reception of their emanationist and demonological world-view, which he should otherwise have attacked were he to have mounted a consistent critique of occultism. Consequently, while he mocked contemporary theories of demons, Champier was unable to deny their existence in an absolute way.

This study does indeed show why the occultist tradition enjoyed respectability and why it could not easily be dispatched: its premises were too much

a part of the sixteenth-century intellectual's *Weltanschauung*. While Champier and others might decry the *magi* and their demons, they could not imagine a universe devoid of Satan's agents. They could only trust, as Champier did, that Christian piety and faith would harness the demons and overcome occultism's popularity, which the pre-Reformation institutional crisis of the Church had helped cause.

In sum, Copenhaver's sensitive study of Champier shows the inexorability of the sixteenth-century reception of occultism. He also shows that the motivation for trying to "disenchant" the world was, at first, religious. The new science eventually overthrew the respectability of the occultist tradition but only after having expurgated the occultism inherent in its own sources.

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EMMANUEL LE ROY LADURIE. *Le Carnaval de Romans: De la Chandeleur au Mercredi des cendres, 1579-1580*. (Bibliothèque des Histoires.) Paris: Gallimard. 1979. Pp. 426.

Again Emmanuel Le Roy Ladurie has focused attention on a single community as a means of coming to a better understanding of the structures and dynamics of a preindustrial society. The subject is Romans, a small city in Dauphiné that during the late 1570s became the foremost center of a widespread insurrection. There an artisan-led coalition of lower orders in 1579 forcibly broadened the base of municipal government at the expense of an oligarchy that had dominated the political life of the city for decades. Noteworthy as this Porchnev-type revolt is, the manner in which the rebels were put down is even more remarkable. The pre-Lenten festivities of 1580 provided a scenario for dramatic confrontations that culminated on the eve of Mardi Gras in a bloody counterrevolution that in turn touched off a general repression of the popular movement throughout the region.

Le Roy Ladurie's demographic, social, economic, political, and cultural analysis of Romans and the surrounding countryside is highlighted against a background of long-developing provincial disputes, the most significant of which were directly related to military and fiscal operations of the Italian and the civil wars. He recounts the main issues of a century-long *procès des tailles* that dated from the reign of Francis I. This set of controversies, concerned primarily with the assessment of direct taxes, produced a common front of *tailles*-paying *roturiers* who were determined to restrict the *taille* exemptions of the privileged orders of provincial society. The Third Estate of Dauphiné was exceptional in developing a

sophisticated theoretical framework for grievances, briefly examined here with reference to some of the *cahiers des doléances* and pamphlets of the period. Also as part of the background Le Roy Ladurie traces the formation of popular "leagues" in the area from 1578 on. Two currents of this originally rural protest movement are identified according to religious orientation. Both were directed against troop depredations, war taxation, and communal indebtedness, both became involved with uprisings in cities of the province, and both led to successful campaigns by combined rural and urban forces against brigand strongholds in the region.

In central chapters on the Romanais revolt and counterrevolt, Le Roy Ladurie presents a fine critique of the principal sources, the most informative of which is an account attributed to the royal judge of the city, a fascinating character who masterfully manipulated pre-Lenten activities to bring about the defeat of his enemies. The political use at carnival time of demonstrations and celebrations by occupational organizations, confraternities, "Abbeys of Misrule," youth groups, and neighborhood associations are described in unusual detail. Most interesting of the carnival events were the *reynages*—contests involving animals which became the symbols under which "kingdoms" were set up that parodied the established order and served to structure both festivities and intrigue. The bold, often brilliant, interdisciplinary interpretation of the symbols, codewords, dances, customary rituals, and spontaneous happenings associated with these "kingdoms" is the most original feature of the work.

Specialists in the history of Dauphiné can quibble with details and argue that a more thorough and systematic treatment of some topics is needed. Even so, Le Roy Ladurie has written one of the best of the monographs on the province, and his study is a valuable addition to the growing body of literature on the uprisings of early modern Europe.

LLEWAIN SCOTT VAN DOREN
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PETER N. STEARNS. *Paths to Authority: The Middle Class and the Industrial Labor Force in France, 1820-48*. Urbana: University of Illinois Press. 1978. Pp. 222. \$12.95.

Peter N. Stearns's new work is a study of the social attitudes and paternalistic actions of the French middle class during what Stearns considers its formative period. He wants to "get at the entrepreneurial mentality" through a study of the relationship between the industrial middle class and workers, because "employers, even those who hoped to view workers only as a factor in production, could not help thinking about them" (p. 107). Stearns there-

fore divides his manufacturers into three "types," corresponding to the degree of their "modernization" as entrepreneurs: "(I) those who refused to change in method or goal, (II) those who changed but imitatively and reluctantly, and (III) those who innovated with a certain degree of zest" (p. 16). The book is primarily about "type III" manufacturers in three industries, who, Stearns contends, improved conditions for their workers out of a combination of entrepreneurial sensibility and concern and thereby laid the foundations of the modern middle-class welfare state.

This is a very mixed book, a somewhat strange combination of solid research and unsubstantiated generalization, of some good ideas and insights joined with ragged conceptualization. Stearns has dipped into an important subject. We need to know more about the bourgeoisie of that period and in general. Like the early utopian socialists, Stearns lumps the industrial middle class with the "producers." Unlike later socialists and many historians, he is their booster, applauding their energy and their efforts to rationalize production. He sympathizes with their difficulties, telling us that "some industrialists had hard lives, even by proletarian standards" (p. 5) and that a few even suffered malnutrition (presumably these were of "type I"). Stearns helps revise the old saw about the non-innovative French entrepreneur, presents an able summary of the development of paternalism and the attitudes of middle-class industrialists toward workers (the best part of the book), offers some provocative comparisons with English cases (however vague), and capitalizes on some interesting sources, such as biographies of industrialists and records of the *conseils des prud'hommes*.

Yet Stearns's paths more often lead us to the present than they take us into the world of class relations during the *monarchie censitaire*. His study bogs down in a framework of crude "modernization." Stearns never really tells us how many "factory" workers there were nor offers a suitable definition of what that entailed in the early nineteenth century. The "factory setting" was not unprecedented and had, with the whole question of industrial discipline, important eighteenth-century antecedents. In any case, as the author implies, factory workers constituted a small minority of workers and in many cases worked alongside traditional artisans. Nor were factory bosses any more representative of the middle class than factory workers were of the labor force. The arbitrary typology is not helpful: there is something unreal about a family "on the border between type I and type II, as entrepreneurs," with a son with "type III ideas" (p. 18). Southern industrialists who opposed the 1841 child labor law (the subject of an interesting discussion) become "non-adapters." Modernization here be-

comes a dynamic that assumes a rigid and rather ahistorical inevitability in its own right.

Furthermore, it is unlikely that the views of manufacturers ultimately "shaped the nature of workers' unrest itself" (p. 178) more than changes in the structure of work and resultant shifts in worker attitudes. Stearns tells us nothing of the impact of the Revolution of 1830 on employers and workers. Even his contention (elaborated in an appendix) that conditions were improving for factory workers is suspect because he overestimates the degree of work stability and depends on wage data, an unreliable index of prosperity if unaccompanied by information on voluntary and involuntary *chômage*.

As with the documentation, the writing is uneven. Parts of the book seem hurriedly written, if not directly dictated to the printer. The bibliography of secondary literature is out of date. *Paths to Authority* will satisfy readers desiring an introduction to the evolution of labor relations during this important period of French history, and it may please those students of contemporary society who care less about understanding the past than uncovering fleeting glimpses of our own time. One can only hope that Stearns will take the time to carry through a full study of class relations and that this study will inspire other research.

JOHN M. MERRIMAN
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MAURICE CRUBELLIER. *L'enfance et la jeunesse dans la société française, 1800-1950*. (Collection U.) Paris: Armand Colin. 1979. Pp. 389.

This work is an ethnography by a cultural historian who uses materials drawn from anthropology, sociology, psychoanalysis, memoir, biography, and popular culture, as well as pedagogy and public record, to demonstrate the impact of the development of a national educational system on childhood and youth in France. In Maurice Crubellier's romantic view of childhood in traditional society, younger generations learned manners, morality, and the means to a livelihood from family, parish, employers, and peers through a slow, spontaneous, organic process. Crubellier asserts, however, that children managed to escape enough from surveillance to form a sort of autonomous youth culture, marked especially by secrecy from and opposition to the adult world. During a century and a half of industrialization and urbanization, solicitous adults discovered childhood, invented "puériculture," and created a school system within which to "recycle childhood . . . [and] integrate it into the cultural circuits of urban-industrial society." The traditional youth culture disappeared, to resurface in part, however, in urban adolescent gangs and the con-

temporary "culture sauvage" of motorcycles, movies, pop music, and blue jeans.

In an engagingly written, richly documented text, Crubellier examines many aspects of the French educational system, including nursery, primary, secondary, and vocational schools, clerical and lay instruction, legal requirements and classroom practices, bourgeois and working-class responses, boys' and girls' schools, sports and games, provinces and Paris. The author believes with Ivan Illich, whom he frequently cites, that all elements of the "schooling society" most assiduously taught a hidden curriculum, the "morale de Franklin," which turns play into work, children into industrious adults, and imposes scientific order on folkways. Purporting to offer the masses access to a higher culture and opportunity for upward mobility, educators of republican as well as religious persuasion in fact presented a profoundly conservative message. Emphasis on a classical course of study and a standardized language of discourse served to maintain the cultural hegemony of the elite. Through the "systematic enclosure of youth" in this "hard world" where new norms in language, manners, and even personal hygiene were taught by rote and sanction, traditional popular culture was diminished and no adequate substitute developed. Educational leaders created only a "museum of values." Deprived of meaningful work and social involvement, youth rebelled and contributed to the state of crisis in contemporary urban society.

Crubellier's work belongs to the literature that calls for reconstruction of educational processes to use rather than repress the "Luddite tendency" of children. It also implies that resolution of the many contradictions between industrial society and humanistic values may be possible, especially with increasing leisure opportunities. Partisans of Illich may use Crubellier to document their call for disestablishment of the schools. Marxists may cite him as evidence of the class struggle in education. Feminists will appreciate his interest in the education of girls and in changing sex roles. All students of the history of childhood, history of education, and French culture will find his new book interesting and useful.

MARILYN J. BOXER
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RAY NICHOLS. *Treason, Tradition, and the Intellectual: Julien Benda and Political Discourse*. Lawrence: Regents Press of Kansas. 1978. Pp. x, 270. \$16.00.

Although well known for his provocative *Treason of the Intellectuals*, the *homme de lettres* Julien Benda has never excited much interest among American students of modern French thought. Benda was per-

haps too narrowly concerned with French issues, and the tradition of the *clerc*, which he defended so vociferously, has no counterpart in American experience. Robert J. Niess (*Julien Benda* [1956]) and now Ray Nichols have tried to convince us of the continuing interest of Benda as a thinker and as an exemplary life. They see Benda as having written the twentieth century's most acute reflection on the permanent tension between the demands of universal reason—man's highest quality—and the irrationality that dominates life in the real world. In this analysis of Benda's thought as expressed in his many published works (his papers, like those of Léon Blum and other Jewish intellectuals, were seized by the Germans and lost), Nichols has relied on Benda's own account of his life. The scholar's analysis of Benda's works largely confirms the writer's self-image, a testimony to Benda's intellectual honesty. To be sure, Nichols is more aware than his subject of certain inconsistencies, of unresolved questions, of polemical exaggerations, and he has given us a nuanced picture of a complex mind.

Nichols is especially strong in explicating *Treason of the Intellectuals* and showing its relation to Benda's other work. After Niess and Nichols there can be no more excuse for simplistic misinterpretations of this celebrated—but more often cited than read—work. Though he defended the ivory tower, Benda was by temperament more polemicist than philosopher, and Nichols is right to conclude that he can best be understood by recognizing that "he was, after all, and above all, a critic" (p. 181), as were so many French Jewish intellectuals.

Nichols's chronological approach allows us to see both the consistency and the development of Benda's thought more clearly than Niess's topical organization; but the older work is not superseded, and some readers will find Niess's humanistic learning more appropriate to the subject than Nichols's social science. Despite their different approaches, they come to remarkably similar conclusions about this irascible gadfly of twentieth-century culture, this "idéologue passionné."

Although the virtues of this well-researched study clearly outweigh its defects, it is unfortunate that Nichols (or his publisher?) felt compelled to begin with an examination of the problem of the "intellectual," an examination that, fortunately, does not prove necessary for the understanding of Benda, who is effectively put into context in the remaining chapters. There are occasional turgid passages throughout, but Nichols's analysis of Benda sheds more light on the question of the intellectual than his introduction leads one to expect. Better editing would also have eliminated some inconsistencies of style in the notes, would have made the author explain the important difference between the com-

mon usage of "pathetic" in English and "pathétique" in French, might have eliminated some minor inaccuracies concerning Péguy's Librairie Bellais, and could have made a fine work out of what is certainly a more than adequate one.

WILLIAM LOGUE
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JEAN-NOËL JEANNENEY and JACQUES JULLIARD "Le Monde" de Beuve-Méry ou le métier d'Alceste. Paris: Editions du Seuil. 1979. Pp. 376.

Le Monde was born of the need in 1944 to fill the "quasi-institutional" gap left by the defunct and compromised *Temps* with a serious, impartial, reference newspaper standing above the plethora of special-interest organs spawned by the liberation. Its founding editors, all approved by de Gaulle himself, included the MRP *pur et dur* Hubert Beuve-Méry, paradoxically a prewar *Temps* correspondent in Prague where he had broken with the newspaper over Munich. In the meantime he had passed through the crucible of the École des Cadres d'Uriage and its redoubtable Pierre Dunoyer de Segonzac, whose style of a reformed monk-soldier was the touchstone of national renewal for many of his followers. Beuve and his companions were given the material resources (including the offices and printing plant of the *Temps*) to guarantee their "moral" independence.

As *Le Monde* grew in readership, it became the mirror of France's most important issues. Rapidly dominating his colleagues on moral and intellectual grounds, Beuve resolutely set the newspaper against the war in Indochina and the alignment of France in the Atlantic bloc. No sooner had the "neutralist" issue of 1950 been calmed, than the "crisis of 1951" broke. René Courtin, one of the original founders, mounted the most dangerous assault on Beuve's authority during the entire history of *Le Monde*, and only the joint persuasion of Maurice Duverger, Jean Monnet, and de Gaulle himself caused Beuve to abandon resignation and vanquish his opponent. In comparison, the attempt in 1956 of an old guard rival with a new organ, *Le Temps de Paris*, to dislodge *Le Monde* was a farce. When de Gaulle came to power in 1958, Beuve forced his approval of the general as "le moindre mal" on his reluctant subordinates, despite his acute awareness of the moral illegality of the coup and his later opposition to Gaullist plebiscitary politics and Maurrassian nationalism. On the twenty-fifth anniversary of his editorship in 1969, Beuve retired, leaving Jacques Fauvet in charge of the newspaper, which was shortly to become the third largest in France and the only one to be entirely controlled by its personnel. *Le Monde* had become what Beuve intended it

to be: nationalist, parliamentarian, and neo-Jansenist.

The authors have used a wide variety of sources, ranging from the archives of and interviews with their protagonists to secondary literature critical of Beuve and his newspaper. They have far more than merely journalistic credentials: Jean-Noël Jeanneney, author of a much remarked study on the de Wendels, is professor of contemporary history at the Institut d'Études Politiques and Jacques Julliard is a professor at Paris-Vincennes. It is thus all the more unfortunate that they should have uncritically adopted the fashionable and tiresome French intellectual shibboleth of an always unspecified American "imperialism" to which France is continually being "inféodé." This myopia does not just prevent them from treating the moral side of a scandal like the publication in 1952 of the sensitive Fechteler report. It also causes them to be less than completely frank on Beuve's desertion of Pierre Mendès-France in 1954, his disregard for the plight of the *pieds noirs* in Algeria in 1958-62, and his guilt-laden handling of the student revolt in 1968.

Can *Le Monde* as an institution become decrepit? Not as long, the authors seem to conclude, as the organ remains faithful to the "conception of information giving" laid down in its first issues. One can only point out that vigilance is necessary, for this certain conception of journalism, like a "certain idea" of France, is not perennially hardy but rather terribly vulnerable to external and internal corruption. Two annexes are included, the more interesting of which portrays the readership of *Le Monde* by sex, age, and socioprofessional category.

PHILIP C. F. BANKWITZ
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JANINE FAYARD *Les membres du conseil de Castille à l'époque moderne (1621-1746)*. (Mémoires et Documents publiés par la Société de l'École des Chartes, number 26.) Geneva: Librairie Droz. 1979. Pp. xxx, 611.

This massive study, based almost exclusively upon exhaustive archival research, is devoted to the social group formed by members of the *Consejo de Castilla* in the period between the *siglo de oro* and the Enlightenment. With the implicit assumption that "le conseil de Castille était le premier corps de l'État" (p. 125), Janine Fayard ignores the judicial, administrative, and political functions of the *Consejo* and examines the family origin, career, wealth, and social rank of its members. The thesis of this examination is that there did exist in Spain—Antonio Domínguez Ortiz to the contrary—a *noblesse de robe*.

From one perspective, Fayard proves her thesis convincingly. Drawn from the *noblesse moyenne* of

northern Castille, the members of the *Consejo* had, by the end of the seventeenth century, been transformed into an endogamous group that had high social status and that perpetuated itself on the basis of family, university, and regional solidarity. In describing and analyzing this group, Fayard provides a fascinating picture of society based upon considerations of "la honra," whose members' lifestyles were dominated by "le besoin de paraître." Although the discussion of the importance of *colegios mayores* only marginally adds to the work of Richard Kagan, that of "le cursus administratif" is excellent. To Fayard—and to the reader—the members of the *Consejo* emerge vividly "comme d'honnêtes praticiens du droit" (p. 538).

But is an endogamous group of jurists with high social status a *noblesse de robe*? By formulating the problem of the existence of a *noblesse de robe* in Spain, Fayard explicitly calls for comparison with the corresponding development in France in the early eighteenth century. Thus, to answer the question affirmatively, one would have first to examine the judicial, administrative, and political power of the *Consejo*, those areas Fayard deliberately ignores. Whatever powers the *Consejo* had in the sixteenth century and was to have again in the early nineteenth century, and whatever power *individual* members of the *Consejo* may have had, it seems clear—as Fayard herself indicates—that the growth of *Juntas* and *validos* in the seventeenth century and that of *secretarios del estado* in the eighteenth century relegated the *Consejo* to a position where its political powers were not equivalent to its social status. And it is only with regard to this last criterion that Fayard can accurately describe the *Consejo* as "le plus beau fleuron" in the administrative system of Spain.

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JOSÉ ANDRÉS-GALLEGOS. *El socialismo durante la dictadura, 1923–1930*. (Colección Historia Política.) Madrid: Tebas. 1977. Pp. 636.

According to the author, who is professor of contemporary history in the extension division of the Spanish university system, this book began as a series of university lectures; consequently, it represents an attempt to synthesize the existing literature rather than to present the results of original research. Such efforts can be useful when the synthesis is the product of thoughtful and prolonged analysis of both primary and secondary materials. Unfortunately, this volume provides little in the way of real synthesis. Instead, it is basically a compendium of facts, documents, and contrasting points of view, most of which have not been sub-

jected to critical evaluation. Nor is there a coherent interpretive framework. As a result, the most valuable part of the book is the four-hundred-page appendix, which conveniently assembles relatively inaccessible official documents and newspaper articles from the period.

José Andrés-Gallego's failure to draw conclusions from his material is not inadvertent but rather grows out of his conviction that the history of the recent past is badly in need of a "dose of positivism" to stimulate "enthusiasm for verifiable facts" (pp. 250–51). It is indeed true that Spanish historiography has often suffered under the twin burdens of political partisanship and a cavalier disregard for hard data. But a significant number of works recently published in Spain overcompensate in favor of a naive positivism in which "the facts" and the documents are left to speak for themselves. The enthusiasm for archival work—so long missing in Spain—is welcome. But the painstaking analysis and synthesis that distinguish history from antiquarianism seem currently to be receiving short shrift.

Despite the author's reluctance to interpret his material systematically, the book nevertheless betrays a certain sympathy for the "collaborationist" orientation of Spanish socialism during the 1920s and an even clearer sympathy for the attempts of the dictator, General Miguel Primo de Rivera, to domesticate the Spanish labor movement. The text deals most thoroughly with the controversy within the Socialist Party and its trade union affiliate, the UGT, over the union's decision in 1924 to accept official positions within the regime as the representative of the Spanish working class, although some of the interesting—and controversial—points raised are too weakly substantiated to be accepted without further research. One example is the author's assertion that the UGT rank and file remained close to the "syndicalist" position maintained by Francisco Largo Caballero, even after he and the rest of the Socialist leadership abandoned it in favor of an "opportunistic" policy of opposition to the dictatorship in August 1929. Others have argued that it was precisely a shift in the mood of the UGT masses that forced the leadership to repudiate its former policy of collaboration.

This controversy highlights the need for more research on the history of the Spanish working classes, as opposed to the history of labor organizations and their leaders. As this volume demonstrates, our knowledge of those organizations remains sketchy. Nevertheless, their impact and significance will only be completely understood when political history begins to be supplemented by the new social history.

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SAMUEL J. MILLER. *Portugal and Rome c. 1748-1830: An Aspect of the Catholic Enlightenment*. (Miscellanea Historiae Pontificiae, number 44.) Rome: Università Gregoriana Editrice. 1978. Pp. xii, 412. L. 20,000.

This thoroughly researched and ably prepared study has two major themes. The first involves a careful tracing of the frequently tense diplomatic relations between Portugal and the papacy from 1748 to 1830. The second is an analysis of the religious and intellectual aspects of the contest between the "regalist" state, determined to enlarge its role in society, a still powerful papacy, seeking to maintain and increase Roman control, and a third group of moderates (whose position was as varied as it was uncomfortable), who sought to create their own vision of the Church.

Samuel J. Miller moves back and forth between these two themes, seeking to show the truly complex relationships between them. When José I came to the throne of Portugal in 1750, the issues of control of the legal business of the papacy, the management of missions in Portuguese territory, and the complex matter of appointing church officers, had been simmering for years. Moreover, the crown was much aggravated by the intransigence and equivocation of the Jesuits toward the Anglo-Portuguese boundary settlement of 1750, in the opinion of many merely the latest and most obvious example of the suspicious independence of the Jesuits.

To these ingredients must be added the nearly simultaneous ascent to power of the future marquês do Pombal and Clement XIII, both powerful and determined men. The result was a clash whose immediate focus was the Jesuit order and which ended in their expulsion in 1759. Within a year, Lisbon broke off official relations with Rome. This situation prevailed until the accession of Clement XIV, in 1769, when there was an abrupt reversal in papal policy, amounting to a substantial victory for Lisbon.

Diplomatic and legal relations were resumed in 1770, but on terms that tacitly recognized the king's rights of *Placet* and appointment. The result was a Portuguese church of decidedly nationalist flavor. Even the death of José I and the fall of Pombal had no immediate effect, and the dominance of regalism was overcome only after the shock of the French Revolution and Napoleonic invasion had frightened and weakened the monarchy and reversed the flow of sentiment.

On the intellectual side, Miller clearly delineates the existence of a moderate Catholic Enlightenment in Portugal—a movement that then and later was carelessly labeled Jansenist but that, in reality, was far more complex. One of the major contributions of Miller's work is to show how inadequate and deceptive it is to label the movement Jansenist. In the

Portuguese context, the Catholic Enlightenment was perhaps principally composed of regalism, episcopal jurisdictionalism, antischolasticism, anti-Molinism, and anti-Jesuitism. The heterogeneous and negative character of the movement is at once apparent, and it was this characteristic that opened it to exploitation by the senior partner, the regalist. Nonetheless, almost no Portuguese reformer, not even Pombal, wished for any total or permanent break with the papacy.

It was just such moderates, backed by Pombal's political power, who carried through the reform of the University of Coimbra, who manned the redirected Board of Censors, and who, in time, were named to high ecclesiastical positions, giving stability and direction to the Portuguese church.

It was the tragedy of all of these parties, however, that in their struggle with each other they lost sight of the real enemy, the antireligious wing of the Enlightenment, that burst on the scene in 1789.

Miller is to be praised for his exhaustive use of a large number and variety of archival sources, for the careful and usually clear delineation of a complex problem (which he knows how to situate within a broad historical context), and, above all, for an ample, judicious, and fair vision of a problem that has been badly obscured by polemics and special pleading. I think this will be the definitive work on the topic.

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PHYLLIS MACK CREW. *Calvinist Preaching and Iconoclasm in the Netherlands, 1544-1569*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1978. Pp. 221. \$19.95.

The author of this work has recognized that historians need to know more about the role of the Calvinist ministers in the Netherlands in the years immediately preceding the outbreak of the Dutch Revolution. Did Calvinist preachers or Calvinist ideology inspire the Dutch Revolt and the Troubles (1566) that preceded it, or were other, perhaps more important, factors involved?

In this book, which is a study of the relationship between ideology and social behavior, Phyllis Mack Crew analyzes the attitudes and character of Calvinist ministers in the Netherlands in the mid-1500s as well as their effect on the popular religious riots and outbreaks of iconoclasm that occurred during the summer of 1566. Crew strives to relate the Troubles to the larger context of the international Calvinist movement and to iconoclastic violence in other countries. In so doing, she questions whether the Netherlands preachers who claimed to be part

of the Reformed community in this period were really as clearly "Calvinist" as historians in the past have supposed, concluding that "the ministers who preached during the Troubles had virtually nothing in common" (p. 100).

Crew maintains that the violence of 1566 was the result of a desire to revitalize communal and personal authority in the face of a collapse in the central political jurisdiction. When that political authority was restored in 1567, the popular movement in turn broke down. Contrary to some older views, she asserts that the Troubles in the Netherlands was neither a symbolic expression of socioeconomic conflict nor the result of a mass conversion to Calvinist doctrines.

Crew has employed the best sources for the proper study of mid-sixteenth-century Netherlands history. Yet, despite her spirited discussion of this oft-neglected corner of Dutch history, several weaknesses make her treatise less than conclusive. First, it is not always clear how she is using certain key words and phrases. For example, what exactly was a "Calvinist" in 1566? This critical definition is absent from her discussion at several points (for example, pp. 40, 47, 57, 65–68, 100, 134, 175). Calvinist ministers, moreover, may have become "a band of revolutionary fanatics" after 1566, but by whose definition (p. 181)? Nor is it always clear against which kind of Calvinism the hedge-preachers of 1566 are being measured—the moderate brand espoused by Calvin himself or the more severe variety of Beza (both are mentioned on page 100). Also lacking clear usage are the important terms "Reformer," "fanatic," "Anabaptist," and "sect."

Second, Crew lacks a firm grasp of the dynamics and basic beliefs of the Anabaptist movement. This is most apparent when she claims that "the theological doctrines of the Mennonites would not have impressed outsiders as radically different from those of other Protestant groups" (p. 148). On the contrary, there were fundamental differences in theology between Anabaptists and other reformers on such issues as believer's baptism, regenerate church membership, and religious liberty. What is more, Anabaptists did not believe in "the idea of spiritual regeneration by adult baptism," as Crew alleges on the same page.

Third, she tends to exaggerate those historical viewpoints she wishes to refute, especially when she maintains, "Both contemporaries and historians have perceived this relationship of ideas and behavior as an either/or situation: either Calvinist ideology inspired the Troubles and the Dutch Revolt, or it did not" (p. 176). Furthermore, she does not identify the historians to whom she alludes.

I am in substantial agreement with Crew's overarching conclusion that, instead of being a rigorously doctrinaire movement, Calvinism contained

more diversity of expression than historians hitherto have generally noticed. Even so, her work, like most doctoral dissertations, is more suggestive than definitive. I, for one, will look forward to her continuing contributions to our knowledge of sixteenth-century Netherlands Calvinism as she hones the interpretations she has put forward here.

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RICHARD W. UNGER. *Dutch Shipbuilding before 1800: Ships and Guilds*. (Aspects of Economic History: The Low Countries, number 2.) Assen: Van Gorcum. 1978. Pp. xi, 216. f 32.50.

This slim volume of 118 pages of text traces the development and organization of the shipbuilding industry and the rules laid down by the Dutch shipbuilders' guilds from the Middle Ages to 1800. Richard W. Unger employs the word "Dutch" to refer primarily to the activity of shipbuilding in Holland and Zeeland rather than to that in the Low Countries as a whole. The use of wind power and design made Dutch ships more effective than those of other countries and laid the foundations for the wealth of Holland and Zeeland in the seventeenth century.

The author traces the rapid growth and development in ship design in the sixteenth and early seventeenth centuries and then the stagnation of the industry in the eighteenth century. Major improvements in shipbuilding practically ceased after 1630. One might wish that the author could state more specifically why this phenomenon took place around this date. The rigid rules of the shipbuilding guilds undoubtedly contributed, as Unger sets forth, but this explanation still leaves one with the feeling that something else must have been involved.

Little is included of any contributions made to the industry by the admiralties of Holland and Zeeland. In the design of ships intended for naval warfare some advances must have been made. The question can be raised whether the Dutch East India Company (VOC), in its construction of ships for the Oriental trade, did not make significant improvements in ship design. It would have been interesting to learn more about the materials used in ship construction, the type of wood, where it came from, and how it was seasoned and prepared.

The chapters dealing with guild organization and regulations are particularly good. In the early period, ship carpenters' guilds were remarkably free from restrictive rules and did not oppressively limit personal freedom. By the eighteenth century, as the author shows, the political activity of the town councils came more and more to regulate the ship

carpenters' guilds. The guilds of the fifteenth and sixteenth centuries on balance fostered growth; the guilds of the late seventeenth and eighteenth centuries hedged innovation in order to preserve jobs. It was the town councils and not the central government that encouraged this type of regulation.

The appendixes include regulations, letters of organization, and bylaws of ship carpenters' guilds from the fifteenth century to 1800. An extensive bibliography of the shipbuilding industry and very helpful footnotes are presented. More illustrations or drawings of various types of ships, sails, and rigging would have been helpful.

This book is highly recommended to anyone interested in studying Dutch maritime history between the Middle Ages and 1800.

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GERALD STRAUSS. *Luther's House of Learning: Indoctrination of the Young in the German Reformation*. Baltimore: Johns Hopkins University Press. 1978. Pp. xii, 390. \$20.00.

There was a greater concern with education in the Renaissance and Reformation period than at any time in history other than our own. In this volume a leading Reformation scholar directs his attention to the question of the Lutheran reformers' efforts to transmit their values to the younger generations during the course of the sixteenth century. Gerald Strauss examines their pedagogical assumptions and expectations, their doctrines and techniques of indoctrination, and assesses their successes and basic failure. The basic assumptions of the reformers were that one must start with the young, that indoctrination is necessary for religious and moral improvement as opposed to academic education, that this indoctrination must be done in public schools and not entrusted to parents apt to be negligent, that teaching will be more effective if based upon a humane fellowship between child and adult, and that religious knowledge and understanding are attainable through the efficient functioning of a well-ordered school system.

The study is based upon the reading of a staggering number of educational treatises, histories of the schools and sermon books, and a very impressive, detailed examination of the reports made by the visitors commissioned by the consistories of the territorial principalities to determine the spiritual and educational conditions in the land. The visitation reports provide less information on the larger cities than on the towns and villages of the countryside, but they contain a vast amount of material of keen interest to the social and religious historian. The author concludes that, in the light of the lofty aims of

the reformers, the evidence of actual accomplishment in improving the common people reveals that their pedagogical venture was a failure. Such success as the reformers enjoyed was possible only because of the support of the state. But that success was so limited when measured against the reformers' purpose of leading all people to think, feel, and act as Christians—to imbue them with a Christian mind-set, motivational drive, and way of life—that their effort must be judged a failure. The antiheroes of this scenario are the Lutheran churchmen, preachers, theologians, and superintendents who lost contact with the common people and their folkish religiosity and promoted abstract theological formulas instead. The Reformation lost its spontaneity after 1530 and became progressively ineffectual as the century wore on. The burden of proof now rests, the author asserts, upon those who claim that the Reformation aroused a widespread, independent, meaningful, and lasting response to its message.

This revisionist thesis makes this book a very important contribution to our picture of the Reformation era. It is rich in interpretive insight and quite unconventional analyses. What can be said in response by a critic who believes that what is said is very true but that the account does not tell the whole story or properly evaluate the positive achievements in religious education? The visitation reports are almost by virtue of their purpose apt to emphasize abuses and failures, "viewing with alarm." A vast body of other literature—devotional booklets for families, aids for catechetical instruction in the household (not just by the state), hymnbooks, prayerbooks, well thumbed in extant copies—suggests a vital religious life among the common people. One hundred thousand copies of Luther's catechism, with its table of duties, were published by 1562 and learned by acolytes and midwives. Parish preachers came from the lower classes and were, in fact, close to the people. The authors of the electoral Saxon constitution urged pastors not to "shame the boys and girls but to treat them in a friendly brotherly manner so that they may look forward to the (confirmation) examination with heartfelt desire and joy rather than fear." In a day such as ours of waning commitment to public education, this superb book conveys a somber message.

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HANS-CHRISTOPH RUBLACK. *Gescheiterte Reformation: Frühreformatorische und protestantische Bewegungen in süd- und westdeutschen geistlichen Residenzen*. (Spätmittelalter und Frühe Neuzeit: Tübinger Beiträge zur Ge-

schichtsforschung, number 4.) Stuttgart: Klett-Cotta. 1978. Pp. 290. DM 88.

PAUL MÜNCH. *Zucht und Ordnung: Reformierte Kirchenverfassungen im 16. und 17. Jahrhundert (Nassau-Dillenburg, Kurpfalz, Hessen-Kassel)*. (Spätmittelalter und Frühe Neuzeit: Tübinger Beiträge zur Geschichtsforschung, number 3.) Stuttgart: Klett-Cotta. 1978. Pp. 232. DM 66.

These studies of sixteenth-century Germany, one conducted on an urban and the other on a territorial base, observe the German Reformation from two unaccustomed angles.

In *Gescheiterte Reformation* Hans-Christoph Rublack investigates the failure of Protestantism to establish itself in eight south and west German cities that served as episcopal sees: Würzburg, Bamberg, Trier, Mainz, Salzburg, Passau, Freising, and Eichstätt. Which "presuppositions, conditions, and supporting circumstances" (p. 4) that were present in those cities where the Reformation succeeded were absent in these bishops' residences? Rublack asks this question both in reference to the early reform movement of the 1520s and in reference to the Protestant minorities that existed in many of these same cities during the second half of the sixteenth century.

The answers are not unexpected. Early reform efforts stagnated at the level of a preachers' movement and were never "politicized." City councils were too weak to provide effective resistance to the bishops who exercised both ecclesiastical and secular power. No permanent links could be forged between longings for urban autonomy and the Reformation. Protestants could thrive only as long as the prince-bishops allowed, and, in the first stage, they tolerated the nascent reform only until the Peasants' War of 1525 supplied a good occasion to suppress it. The tantalizing question, *why* the preachers in these cities could not mobilize the citizenry and the councils against the bishops, is not answered. Rublack hints that the answer might lie partly in the timidity of the preachers and partly in the submissiveness of the citizenry. One might also ask what social and economic conditions were lacking that sparked the urban revolutions accompanying the successful breakthrough of the Reformation in many north German cities.

Zucht und Ordnung investigates the rise, structure, and sociopolitical function of the Reformed church orders in Nassau-Dillenburg, the Electoral Palatinate, and Hesse-Kassel. These German Reformed churches did not adopt a completely Calvinist structure like their kindred churches to the west but retained constitutional remnants from their earlier Lutheran phase. The historical development of the Reformed orders and a comparative study of their elements lead to the same conclusions. First, the conversion of these territories from Lutheranism to

Calvinism did not curb the tendency of the secular ruler to exercise control over church affairs. Second, questions of polity did not play a decisive role in the changeover to Calvinism; rather, dogmatic concerns more vital to the theologians and liturgical practices more visible to the people dominated.

In his analysis of the socio-political function of the orders, Paul Münch elevates the concepts of *decorum* (which he translates as *Zucht*?) and *ordo* to components of a theory of social control and concludes that Calvinism promoted more discipline than democracy. This theory deserves to be tested on a broader textual basis than the preface to the Church Order of Hesse (1566). If true, it would reinforce what Münch himself suspects (p. 191): Calvinism's contribution to democracy had more to do with the external situation of its churches under persecution than with its internal theory.

Taken together, these studies document an important and sobering ingredient in the total Reformation picture: in spite of its epochal character, Protestantism failed to penetrate major centers of Germany. Even where it did succeed, it often failed to alter political and social realities, although it passed through phases where such alterations theoretically should have resulted. The abundant, informative material presented in these works, partly based on archival sources and overflowing into extensive appendixes, still does not fully explain these failures.

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WINFRIED SCHULZE. *Reich und Türkengefahr im späten 16. Jahrhundert: Studien zu den politischen und gesellschaftlichen Auswirkungen einer äusseren Bedrohung*. Munich: Verlag C. H. Beck. 1978. Pp. xvi, 417. DM 88.

This excellent monograph assesses the impact of an external threat, the great Turkish "menace," on the internal history of the Holy Roman Empire during the latter half of the sixteenth century. While the temporal focus is justified both by the intensity of the military danger and by a relative neglect of the period in previous research, it is no limitation on the author, who casts a wide analytical net over the entire "long" sixteenth century. No work on the Turkish threat is more important than Winfried Schulze's, precisely because he has had the imagination to realize the full potential of the subject. Furthermore, the book exemplifies the current conception of constitutional history in Germany, firmly anchored in careful legal and institutional analysis but also comprehending the power struggles and social relations behind formal political structures. Though the school has not moved close to the "new" social history in its attempt to understand group behavior or mentalities, it has developed a

genuine concern for lower social orders as well as elites. More impressively, it has infused the study of public life with a new sense of political reality derived from a full recognition of the role of social conflict in politics and from analysis of the actual *functioning* of institutions. Schulze is a leading representative of this approach, and he has chosen a theme that elucidates many issues in early modern German history.

The Turkish danger was a real one, of course, but was most easily perceived by the governing elites, who led the fight against it and had the most to lose from defeat. They saw popular indifference to the Turks as an "internal threat," which they sought to overcome by creating a horrifying image of the "hereditary enemy" and by preaching a united front to save the Empire and Christendom itself. This ideology sought to stabilize the internal political and social order at a time when it faced not only a military threat abroad but also religious enmity and serious popular unrest at home.

The interplay between external and internal considerations is seen throughout Schulze's analysis of imperial politics, an analysis that confirms—even for the late sixteenth century—the emphasis in recent research on the viability of the Empire. His discussion of the Reichstag, for example, shows the political astuteness of the Habsburgs and reveals that even the most dissatisfied Protestant leaders wished to preserve imperial institutions during this period of acute danger. Unable to gain redress of their religious grievances, the Protestants were nonetheless willing to authorize "voluntary" contributions to campaigns outside the Empire. Schulze devotes most attention to imperial finances and demonstrates Habsburg success in developing a well-functioning financial administration and in collecting over 80 percent of the money appropriated for defense against the Turks. This whole issue gives Schulze a forum for a penetrating analysis of taxation at both the imperial and territorial levels over the late fifteenth and sixteenth centuries, the most intelligent summary available. The discussion of the transfer of the tax burden from the territorial lords to their subjects is yet another example of the analytical richness here. Schulze's fine book is essential reading for all students of early modern Germany, indeed of sixteenth- and seventeenth-century Europe.

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RÜDIGER SCHÜTZ. *Preussen und die Rheinlande: Studien zur preussischen Integrationspolitik im Vormärz*. Wiesbaden: Franz Steiner Verlag. 1979. Pp. vi, 263. DM 56.

When Prussia acquired its extensive Rhenish territories at the end of the Napoleonic wars it inherited

a serious political and administrative problem: how to integrate this western area, which had lived for two decades under a French administration that stressed civic equality, into the Prussian kingdom, which, despite some modernizing reforms, was still organized and governed in the spirit of the old regime. The Prussians were shocked when their well-meaning attempts to introduce good old Prussian laws and institutions into the new provinces were resisted by a populace grown fond of the equalitarian aspects of French rule. How were they to create a real union, a Prussian nation, without some consistent policy regarding the legal status of Prussian subjects? They listened, sometimes, to Rhenish objections. They delayed and temporized, and they made concessions. But in the end they extinguished the most significantly divergent elements of Rhenish multiformity. They eventually Prussianized the Rhineland.

Rüdiger Schütz sets out to investigate this process of Prussianization. He examines the office of provincial *Oberpräsident*, an anomalous third level of administration that might have developed the strength to defend provincial autonomy but never did. He traces the heated arguments over local government from the Rhineland's rejection of Stein's *Städteordnung* up to its grudging acceptance of the *Gemeindeordnung* of 1845, which contained some major concessions to Rhenish peculiarities—concessions that were largely nullified a few years later. And he examines how the king of Prussia backed off of his promise to create a Prussian national parliament after he discovered that Rhenish conditions would not accommodate the *altständisch* foundations upon which he hoped to base it.

Schütz's account of these matters is disappointing. In each case he briefly describes the initiatives from Berlin, and then he reports at great length the negative reactions of Rhenish officials, mostly the *Oberpräsidenten* and the *Regierungspräsidenten*. Finally, he tells the reader how it all came out in the end, the policy ultimately adopted in Berlin. But he makes little attempt to explain why the Rhenish officials adopted their negative positions. What were the experiences, the concrete political pressures that led them to defend the Rhenish status quo against Berlin's centralizing tendencies? Schütz never gets far enough outside of the bureaucrats' offices to be able to tell us. His history is strictly *innerbehördlich*, as he says, and that approach has obvious and severe limitations. Furthermore, in most cases it appears that Rhenish arguments had little if any impact at all on Berlin. The ministers seem to have made their decisions usually on the basis of other considerations. It is not clear why one should care particularly about what the Rhineland officials were saying when nobody was really listening to them.

The book can best be understood as a companion to the same author's *Grundriss zur deutschen Verwal-*

tungsgeschichte, 1815–1945 (1978). That work is basically a collection of documents illustrating the administrative history of the Rhineland. This book, too, is primarily a set of documents—namely, the memoranda and reports of the Rhenish officials. These documents are not simply reproduced, of course, but a large and important part of the text consists simply of paraphrases of them, sometimes in stupefyingly superfluous detail. No doubt this provides valuable raw material for a penetrating study of Prussian Rhineland policy in *Vormärz*, but that study has not yet been written.

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DIRK BLASIUS. *Kriminalität und Alltag: Zur Konfliktgeschichte des Alltagslebens im 19. Jahrhundert*. (Kleine Vandenhoeck-Reihe, number 1448.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 95. DM 9.80.

During the first half of the nineteenth century Prussia experienced the onset of modernization. The economic and social values of capitalistic individualism were increasingly accepted by the governing elites; the state was enlarging its demands on the population, and its bureaucracy was growing in both scope and effectiveness; economic life entered that awkward transitional phase between the downfall of traditional structures and the full arrival of modern industrialism. It is within this context that criminality in Prussia—understood as the number of recorded criminal investigations—rose dramatically, well beyond what might be attributable simply to more efficient law enforcement. In an earlier work Dirk Blasius examined the phenomenon primarily from the standpoint of the reform of criminal law and the judicial bureaucracy. This new essay recapitulates the essence of the earlier argument but focuses more on the perpetrators of crime and their motivations.

Blasius is interested chiefly in petty criminality—small thefts, especially thefts of wood, evasion of taxes or customs, insubordination, arson, and so on—and how it illuminates the condition and mentality of the masses. In general, two types of linkages can be posited: the economic and the political. Economic causality has often been stressed and is easily demonstrated. Pauperism and deprivation were widespread in Prussia in the decades after the Napoleonic wars. That petty criminality, especially theft, could be the product of economic desperation is readily believable. The correlation between the price of grain (rye) and the number of criminal investigations between 1836 and 1865 is very high (Pearson's r exceeds +0.90).

But Blasius finds political causality more per-

suasive in this case. Indeed, he argues that petty criminality during these years should be seen as popular rejection of the norms of the emerging capitalist industrial state and as a primitive form of proletarian class consciousness and solidarity. Evasion of taxes or customs duties and insubordination when confronted by an official represent the unwillingness of the masses to recognize both the authority of the reformed state and the validity of the new legal situation. Similarly the extremely high incidence of thefts of wood—more than three-quarters of all thefts—represents the resistance of the rural poor to capitalist property rights that denied them their traditional rights of gathering scrub wood. Blasius also claims that, contrary to the views of other authors, thefts do not indicate an increase in the valuation of bourgeois property on the part of the masses but rather the growing resentment of inequality. Moreover, he argues that the importance of thefts as an indicator of the onset of industrial society has been overrated. In Prussia the linear trend of thefts does increase between 1836 and 1850, but then it decreases between 1852 and 1866, the period of industrial take-off. Meanwhile, the trend for crimes involving bodily injury increases during both periods. From this Blasius concludes that crimes of assault are the real characteristic of modern criminality in the urban, industrial age because they “reflect the psychic damage inflicted on the way to modern society” (p. 50).

The evidence presented here to support these assertions is rather meagre. Undoubtedly the essay's shortness precluded the inclusion of more details, and it is hoped that Blasius will document his interpretation more fully in a subsequent study. Nonetheless, the present work can be recommended as a stimulating and thought-provoking introduction to the social history of criminality in nineteenth-century Prussia.

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LAWRENCE SCHOFFER. *The Formation of a Modern Labor Force: Upper Silesia, 1865–1914*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xvi, 213. \$13.50.

This monograph traces the development of the labor force in the Upper Silesian mining and manufacturing region during the period of its most rapid growth. It examines the evolution of a population of unskilled ex-peasants into a force of disciplined, committed, “modern” industrial workers, divorced from the land and able to influence conditions of employment through organization and other tactics.

Though at first glance a short book, it is quite

rich both in material and in the variety of questions it touches upon. One of its principal themes is the chronic labor shortage that Upper Silesia experienced during the period under examination. Although situated atop one of the richest coal fields in Europe, this region was unable to compete for labor with the Ruhr and other German industrial areas. It drew its labor force from a surprisingly small area, essentially the immediate region itself. The area's predominantly Polish ethnic character apparently acted as a deterrent to immigration by German workers, while tight control of immigration from Russia and Austria-Hungary (due to concern about the national balance in Germany) prevented the tapping (except on a seasonal basis) of the large labor supply in the Slavic east. Despite this labor shortage, wages remained lower, hours longer, and conditions generally less favorable than elsewhere in industrial Germany; a response in the form of strikes and other union activities came only on the eve of World War I. Lawrence Schofer suggests that workers learned to exploit their relative scarcity earlier: he sees the high levels of absenteeism and job-hopping as tactical moves in this struggle and not as indications of a lingering anti-industrial ethos. But, in the absence of evidence of conscious intent to use such behavior strategically (for example, explicit statements by workers themselves), this interpretation must remain largely conjecture.

Schofer has ranged widely in a number of fields besides history, is familiar with the Polish and German literature, and has amassed an impressive amount of information. To be sure, his work has its share of shortcomings, the most serious of which is the general neglect of the national dimension. Among the variables that affect the applicability of developments in Upper Silesia to general labor history and theory, this is certainly the most obvious and important. Schofer largely ignores the problem of the ambivalent national identity of Upper Silesian Poles—the partial development of a Polish national-political consciousness (in competition with Catholic, socialist, and traditional Prussian world-views) in the midst of increasingly aggressive nationalist policies from Berlin, which dominated most aspects of life in the Prussian east at this time. There is also a regrettable absence of the “subjective” angle, a lack of first-person accounts by workers to compensate for the somewhat bloodless picture one creates when trying to reconstruct the life of any group from official statistics. As a result, due to the imperfect quality of the available evidence or its selective use by the author, one is left with a rather incomplete picture of the Upper Silesian work force. More information on the general state of the local economy—for example, the profit picture and other aspects of management's point of view—would also be helpful. There are frequent

suggestions that Upper Silesian entrepreneurs and managers were less than competent, as slow as their workers to adopt more rational forms of industrial behavior, but we are given little evidence with which to address such issues.

For the most part, Schofer is a social historian of the right kind: open to stimulation from the often rarefied world of social “science,” willing to give its notions a try but faithful to the primacy of historical fact (as we know it) over abstract constructs. But there remains a disparity between the quite good material he presents and his conceptual framework. The latter consists of a somewhat ill-defined concept of a “modern labor force” and an idealized view of optimal labor-management relations toward which modern economies are presumed to be moving. On the one hand, the data remain too limited and incomplete to support satisfactorily the ambitious model; on the other, the model is inadequate to handle all of the material that is presented. This inadequacy leads to numerous tangents and causes the sources to determine the book's contents in many cases. Thus, it is not a particularly easy work to read, and locating the numerous footnotes at the back of the book makes reading as much an exercise for the hands as for the eyes.

Overall, historians will probably value this work more for its considerable informational content than for its conceptual thrust. It is certainly a solid research effort that future students of the Upper Silesian situation will be obliged to consult.

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SHULAMIT VOLKOV. *The Rise of Popular Antimodernism in Germany: The Urban Master Artisans, 1873–1896*. Princeton: Princeton University Press. 1978. Pp. ix, 399. \$25.00.

Shulamit Volkov cuts through a persistent ambiguity in studies of European antimodernism by concluding that, “as a reaction to modernity, antimodernism was itself a modern phenomenon” (pp. 326–27). She reaches this position through a careful and sustained examination of the history of German small independent urban master artisans practicing what she terms “traditional crafts” in the last quarter of the nineteenth century. Volkov blends the now familiar psycho-intellectual *Fragestellung* about German antimodernism with very solid social analysis, based on archival research. Following the lead of Heinrich August Winkler, she brings the discussion down to the shop floor. In so doing she renders a necessary criticism of earlier “intellectualist” studies such as Fritz Stern's and my own.

She has ransacked the appropriate archives at

Potsdam and Merseburg as well as looked at selected regional sources of interest. She compares English with German industrial history to illuminate her arguments about the special nature of the German development. And she operates on a level of theoretical sophistication unusual, and welcomed, in a historical work. All of this is to say that Volkov learned her craft in Hans Rosenberg's workshop and that the 1873-96 depression frames her analysis.

In the odyssey of the small urban artisans from the 1840s, when they were open to various political orientations, to the 1920s, when many aligned themselves with National Socialism, she argues that the period sometimes designated the "great depression" marked the crucial changes in their collective biographies. Modernization before the 1870s had distressed many masters; others, as she well points out, benefited from it. But all had to adjust in some ways to Germany's emerging industrial society. In the 1873-96 period, however, "depressed prices, a decelerated rate of economic growth, and considerable material hardship" (p. 60) seemed to reduce their ability to respond rationally to their circumstances, in Volkov's view. The master artisans felt themselves outside the new society: they were offended by the industrial capitalist social structure growing up around them; they were wounded by their journeymen treating them as bosses rather than as fatherly mentors. Until the 1870s the urban artisans looked regularly to the liberal bourgeoisie for political direction and alliances. The "great depression" broke this tie and introduced the "Appeal of the Extremes." Yet, the artisans did not simply give themselves up to some extremist group; rather, they turned inward, hoping to cultivate a sense of corporate identity—which, of course, deepened their political isolation. Attempts at playing the game of interest-group politics failed. Only the irrational options remained, according to this argument. Artisans in increasing numbers succumbed to the lures of anti-Semitism, authoritarianism, monarchism, nationalism (p. 302), anticapitalism, and antisocialism (p. 310). It seems to me inaccurate to characterize this complex dialectic of artisans' ideas, hopes, and fears as simply the inspiration of emotion rather than of logic, as she does (p. 310).

But, if the masters dealt with their reality irrationally, they were encouraged by "conservatives hoping to exploit the Masters' antimodernism for their own ends": namely, "the last effort of the Prussian agrarian aristocracy allied with the big business oligarchy to preserve a social supremacy long outdated and even more precarious" (p. 325). After the war this second-line defense of the elite passed to the lead—behind the leadership of the National Socialists, of course. Thus, in the ongoing debate about the role of fascism in advancing or retarding modernization, Volkov has taken the side of those

who, to paraphrase Arthur Rosenberg, see the Meister Johann Kleinermann as the culprit of National Socialism. He was driven to irrational extremes as a more or less incomprehending victim of "modernization."

Volkov reaches into an old social science portmanteau and finds the concepts of "traditional" and "modern" society. Since the concepts have served as receptacles for reductionist judgments about extremely varied metropolitan and peripheral societies, she might have been more cautious about applying them to the curious commingling of old and new social institutions and patterns in late nineteenth-century Germany—or, indeed, late nineteenth-century anywhere. They do not advance her argument; they paper over problems. It is true that most of the fascist movements of the twentieth century contained what she labels "antimodern" elements, but so indeed does a July 4 or 14 oration by a modern bourgeois politician linking himself to national traditions on the advice of a market research firm. Were the Italian Futurists or the students in the Iron Guard antimodern? Do present-day leaders of corporate capitalist planning owe nothing to the fascist era, to Johann Kleinermann's horror at the capricious play of the market?

Finally, we have to begin to exercise more caution before the temptation of using the "great depression" as a hanger upon which we place the important events of the 1873-96 period. Volkov admits that the aggregate small business sector grew at a rate equal to the German economy as a whole in the crucial period (p. 71). And, although she does take great care to correlate some artisan activity with smaller cycles within the large one, there seems more symmetry than explanation in the effort. For all of the excellent scholarship Hans Rosenberg's approach has engendered—and this is a book of excellent scholarship—it comes at a price. Volkov must systematically eschew the mechanical economic determinist temptations of the "great depression" theory. The theory operates in her book more as a classification scheme than as an explanation. For explanation we are proffered a poor "modernization" model.

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RAINER KOCH. *Demokratie und Staat bei Julius Fröbel, 1805-1893: Liberales Denken zwischen Naturrecht und Sozialdarwinismus*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz. Abteilung Universalgeschichte, number 84.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. xiii, 298. DM 72.

Julius Fröbel belonged to that generation of Germans whose lives were bisected by the revolution of

1848. He was born in 1805, grew up in a Thuringian *Kleinstadt*, gravitated toward radical politics as a young man, and supported himself by working in journalism, publishing, and on the fringes of academic life. The great events of March 1848 seemed to open a new world of opportunity for Fröbel, his party, and his nation. During the revolution, he was active in democratic organizations, served in the Frankfurt Parliament, and finally was forced to flee into exile. After an adventuresome career in the United States, Fröbel returned to Germany, reconciled himself to Bismarck's Reich, and served for a time in the Consular Corps. When he died in 1893, he was an advocate of suffrage restrictions, social Darwinism, and Realpolitik.

Although Rainer Koch has some interesting things to say about Fröbel's life, his major concern is with his subject's ideas. Koch believes that these ideas—largely contained in a series of books and pamphlets written in the late 1830s and 1840s—have two claims on our attention. In the first place, Fröbel's attempt to build a theory of democracy on the “corner stones of freedom, equality, and participation” was more successful than any of his German contemporaries (pp. x–xi). Second, Fröbel represents an essential link in the long but elusive tradition of German democratic thought. In order to define this historical and contemporary relevance, Koch applies a method based on Max Weber's notion of the “ideal type.” The product of this method is not a summary of Fröbel's thought but rather an abstraction of its most essential elements, to be presented with a fullness and consistency greater than Fröbel himself might have been able to accomplish (p. 175).

The basis of Fröbel's achievement as a democratic theorist, Koch argues, was his uncompromising demand that sovereignty be located in the people. Suffrage regulations, representative institutions, and the legal system must provide the broadest and richest involvement of the nation in the business of government. At the same time, Fröbel recognized the potential dangers involved in a democratic polity. To avoid these, he insisted upon the protection of individual rights and property as well as upon social reforms to reduce poverty and discontent. In sum, Fröbel sought a dynamic and creative synthesis of popular participation and the protection of individual rights (see Koch's summary on pp. 142–43).

While I have no doubts about the value of this account of Fröbel's ideas, I am uncertain as to how much Koch's accomplishments are due to his use of the “ideal type.” Much of this book looks to me like old-fashioned summary and analysis. Moreover, I am uneasy about a method that seeks to present ideas without their inconsistencies and omissions—it is often precisely these elements that tell us the

most about the internal strains and tensions of an intellectual construction. Finally, I am not sure one can or should construct an ideal type of Vormärz democracy from a single individual's ideas. Typicality cannot be conferred on one's subject a priori but can only come from a careful comparison of his ideas with those of his contemporaries. Until Koch turns himself to this task, we shall have to be content with the more limited but still highly rewarding in case study he has given us in this book.

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ROSEMARIE LEUSCHEN-SEPPEL. *Sozialdemokratie und Antisemitismus im Kaiserreich: Die Auseinandersetzungen der Partei mit den konservativen und völkischen Strömungen des Antisemitismus, 1871–1914.* (Politik- und Gesellschaftsgeschichte.) Bonn: Verlag Neue Gesellschaft. 1978. Pp. 347. DM 72.

This study of the German Social Democratic Party's position on the Jewish question in imperial Germany proceeds from the premise that theoretical formulations by Marx, Kautsky, Mehring, and others were less important formative influences than the ways in which local party activists and the popular party press dealt with the problem of anti-Semitism. Anyone familiar with Paul Massing's classic study, *Rehearsal for Destruction*, will find little here to change his mind about the official Social Democratic analysis of Judeophobia. What is new and insightful here is an examination of the attitudes and actions of lesser party members and publications.

Rosemarie Leuschen-Seppel shows that the unity of theory and practice displayed by the Social Democrats in their opposition to political anti-Semitism during the 1880s broke down after the anti-Socialist law was dropped and the Stoecker movement collapsed. Marxist theorists continued to condemn all forms of racism, but they belittled post-Stoecker anti-Semitism as a bourgeois affliction that left the workers untouched. Local and regional party leaders, however, carried forward their militant confrontation with anti-Jewish agitators into the 1890s. Of more lasting significance was the growing gulf between Socialist denunciations of the social consequences of anti-Semitism at the political level and occasional employment of unflattering caricatures of Jews at the cultural level. The latter is painstakingly documented in influential (but today frequently ignored) publications designed to amuse and entertain German workers and is illustrated with examples in the appendix. It is attributed principally to the party having underestimated the importance of evolving a true proletarian culture, which left workers and party members easy prey to

integration by state institutions into existing German culture, complete with its racial prejudices. Acculturation was aided by the Socialists' smug assumption that anti-Semitism was on the way to extinction, which encouraged a certain opportunistic toleration of racist symbols, and by the Socialists' unacknowledged nationalism, which deepened their identification with Wilhelminian Germany's economic and cultural achievements. The author concludes that this simultaneous defense and negative stereotyping of the Jews generated working-class ambivalence toward the Jewish question.

Ambivalence there certainly was, but not everyone will agree that its main roots were in proletarian acculturation. Social Democrats had reasons for criticizing Jews that were uniquely their own, not the least of which was annoyance over Jewish politics. They believed that a persecuted minority like the Jews had a special obligation to support Social Democracy, the party of all the oppressed, and it must have seemed to them a monumental case of ingratitude that most German Jews stubbornly remained outside the Socialist camp long after it had proved itself a mighty bulwark against anti-Semitism. That criticism aside, this study deserves high marks for its sensitive and thorough treatment of a difficult topic. It should help put to rest the myth of an anti-Semitic tradition in German Socialism, without in any way contributing to the equally misleading view that Social Democrats were uninfluenced by anti-Jewish sentiments.

DONALD L. NIEWYK
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ROLF WEITOWITZ. *Deutsche Politik und Handelspolitik unter Reichskanzler Leo von Caprivi, 1890-1894*. Düsseldorf: Droste Verlag. 1978. Pp. vii, 406. DM 48.

The author, who received his degrees from the London School of Economics where he worked under James Joll, has constructed a detailed account of the planning and negotiation of the Caprivi trade treaties, based on thorough use of archival materials in Bonn, Coblenz, Potsdam, Merseburg, and Munich. His investigations do not lead him to new political interpretations but rather confirm previous judgments. Caprivi's trade negotiations were an integral part of his "new course" policy of rational adjustment and reconciliation, and, as with his other policies, both domestic and foreign, the overall result was disappointing. Because no fundamental change in the political structure of the Reich was envisaged, Caprivi's reform gestures were doomed to failure from the outset. Lowering the price of bread by reducing the grain tariff and helping to provide jobs and higher wages by encouraging exports did not give the masses the political recogni-

tion that they sought. Rolf Weitowitz judges that the economic impact of the treaties was at best secondary and only a minor assist to a world boom from 1895 to 1914 in industrial production and trade that would have occurred anyway. German trade with the Triple Alliance powers, Austria and Italy, did not increase; the greatest advance in German trade was with Russia, where trade in items with continuing high import tolls increased as much as in those products with reduced rates. Socially, the trade treaties probably assisted in the "negative integration" of the Socialists into German society before World War I, but they also set the style among the ruling groups of looking for "ersatz solutions" to social and political problems instead of real ones. Also, and possibly most importantly, by alienating and provoking the ruling agrarian Junker conservatives, Caprivi not only brought about his own political downfall, but his rational trade policy encouraged them politically to develop an irrational "pre-fascist radicalism." This reviewer must add that Caprivi, as a military man, shared the technician's approach to politics with other civilian members of the bureaucratic German government; that is, he saw the trees but not the forest; his approach was essentially apolitical. Also, the authority of the government could have been brought to bear more effectively, more significant reforms might possibly have been accomplished, and the more rabid Junkers could have been held in check through the medium of a more genuinely liberal and responsible head of the authoritarian state structure, instead of the neurotically posturing William II whose personality accentuated most of the problems of the second German Reich.

J. ALDEN NICHOLS
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GERHARD PADDERATZ. *Conradi und Hamburg: Die Anfänge der deutschen Adventgemeinde (1889-1914) unter besonderer Berücksichtigung der organisatorischen, finanziellen, und sozialen Aspekte*. Hamburg: By the Author. 1978. Pp. vi, 298.

Gerhard Padderatz's volume written in German was originally a dissertation for his doctoral degree at Kiel University in Germany. It is an interesting and well-documented study of the Seventh-Day Adventist Church in Germany and the central role played by Louis R. Conradi in this endeavor. Unfortunately many of the church's documents were destroyed in Hamburg during World War II.

The volume begins with the Millerite movement of the 1840s in the United States, and it describes how the Seventh-Day Adventist Church grew out of this movement. Padderatz emphasizes the influence of Ellen G. White, one of the church's found-

ers, in the development of the early church in the United States as well as in Germany. The author explains how the church was built up in Germany, its triumphs and failures, and Conradi's importance to the movement as the major motivating force. The most important reasons for the success of Adventism in Germany were the distribution of Adventist publications and the holding of evangelistic meetings. Since the church was a new sect and made proselytes from other denominations, considerable opposition to it developed. Other problems that are treated in the book are the relationship of Seventh-Day Adventist young men to the military service, the problem of school attendance on Saturday, and opposition of the civil authorities to the holding of public meetings by Adventists in certain provinces. What is enlightening and shatters the lofty image most members had of Conradi was the revelation of his frailties and mistakes and the problems they caused to the church.

The book leaves many questions unanswered, has certain flaws, and therefore is an incomplete account of Conradi and the Adventist Church in Germany. Padderatz resorts to speculation when he offers no documentation to the charge that the European General Conference was dissolved because of Conradi's moral failure. His research is suspect when he says that no Adventist German bore arms before 1914 and that the only reason they got into difficulty with the military authorities was their refusal to work on Saturdays. This is erroneous and is refuted by authorities in the field.

The question is raised why the author ignored any discussion of the change of church policy concerning military service that took place once Germany was involved in World War I. This is disappointing since it would be very enlightening concerning the character of Conradi and the real nature of German Adventism. His mistake was to take unilateral action approving the bearing of arms for German Adventist men, which was contrary to the official position enunciated by the denominational headquarters in the United States. Conradi was later reprimanded for this unwise decision, and after the war he repudiated his action. German Adventists like to forget this unfortunate episode in their history.

The book is helpful to the novice in giving a broader understanding of the general nature of the development of the Adventist Church in Germany but does not add significantly to historical knowledge. Furthermore, it gives no penetrating interpretations of the significance of the various developments of the Adventist Church in Germany in relation to other denominations and to the communities in which the churches were located.

JACK M. PATT
San Jose State University

RÜDIGER ZIMMERMANN. *Der Leninbund: Linke Kommunisten in der Weimarer Republik*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 62.) Düsseldorf: Droste Verlag. 1978. Pp. 307. DM 64.

The shelf of books on factionalism and schism in the working-class parties of the Weimar Republic is growing long. The subject matter is certainly there: in Germany's troubled years from 1917 to 1932 there were at least eight significant splits in the workers' parties (four in the Communist Party alone). Establishing the particulars of the splits and of the new organizations growing out of them is often a formidable research exercise, with very high standards set by Hermann Weber and others. Yet with the present book—which treats the course and aftermath of a Communist schism of 1926–27—we now have detailed scholarly treatments of all these affairs except the short-lived Communist schism of 1921–22. It will soon be time for someone to draw an integrated picture of the phenomenon.

It is no accident that the Communist schism of 1926–27 should be one of the last to find its historian: it was one of the most confused and least promising. The group headed by former party eminences Ruth Fischer, Arkadij Maslow, and Hugo Urbahns, which was later to found the Leninbund, was forced out of the party at about the same time as a number of smaller "ultraleft" factions (led by figures like Karl Korsch), which never coalesced with the larger faction or with each other, but all criticized the KPD from the left. Then the Leninbund itself proved highly fissiparous, losing Fischer and Maslow (and much of the membership) immediately after its foundation early in 1928 and losing further cadres to the new Trotskyite movement in 1929–30. It had perhaps six thousand members at its foundation, which was also its peak, and fewer than a thousand in the years from 1930 to the end of the republic. Its ideology, much of the time, was conspicuously futile, and its impact on public affairs generally negligible.

Rüdiger Zimmermann has done the work of sorting out the facts carefully and well. In fact, though he writes with unusual (for German scholarship) simplicity and directness and offers pungent commentary as the narrative proceeds, the main part of his story is so detailed that even most specialists will probably use the work only for reference (though reference use is impeded by the lack of a subject index). Fortunately, the author has more on his mind than establishing the facts. In passages of more general interest—particularly the two evaluative chapters at the end—he presents his characters as representative of certain dilemmas of dissident Communists, particularly dissidents who remain loyal to the Leninist conception of a cadre-party

and to an ideal vision of the promise of the Soviet Union. The Leninbund faced, rather helplessly, the twists and turns of Stalinization in Russia and the Comintern in 1926–29; it also faced, sometimes with real penetration, the danger of fascism in Germany. From both confrontations there are general insights to be gained, and Zimmermann succeeds in bringing them out.

DAVID W. MORGAN
Wesleyan University

HANNSJOACHIM W. KOCH. *Der deutsche Bürgerkrieg: Eine Geschichte der deutschen und österreichischen Freikorps, 1918–1923*. Berlin: Ullstein. 1978. Pp. 487.

Another book on the Free Corps? In fact, as Hannsjoachim W. Koch notes, there have been only two major studies of the Free Corps since 1945, Robert G. L. Waite's *Vanguard of Nazism* and Hagen Schulze's *Freikorps und Republik*. But this is somewhat misleading, since other authors, most notably Harold J. Gordon and F. L. Carsten, have written extensively on this subject within the context of larger works. Nevertheless, an updated, comprehensive history of the Free Corps would be of value. Koch's attempt to provide such a history, unfortunately, succeeds only in part.

Der deutsche Bürgerkrieg consists of twelve chapters covering the formation of the Free Corps, their suppression of revolutionary unrest in Germany, Free Corps campaigns in the Baltic, Upper Silesia, and Carinthia, the Kapp Putsch and resultant Ruhr uprising, and, finally, the dissolution of the Free Corps and their subsequent underground activity, culminating in the violent events of 1923. Objecting to Waite's "tendentious" treatment of the Free Corps, Koch presents a more sympathetic—yet generally accurate and convincing—account of the Free Corps' mentality and the feelings of anger, disillusionment, and betrayal that eventually turned many members equally against the young democratic republic and Wilhelmine conservatism.

While providing a broad overview of Free Corps activity, the book on the whole adds little that is new or significant to what is already known. The chapter on the Austrian Free Corps in Carinthia provides some interesting material, but Koch's rather narrow diplomatic-military focus limits its value as a basis for a meaningful comparative analysis. Similarly, the new archival material introduced by Koch, since it originates primarily from diplomatic archives, helps to flesh out the international complications surrounding Free Corps activities but does little to augment the existing picture of the Free Corps themselves. Finally, although Koch, in contrast to Schulze, carries his story beyond the formal dissolution of the Free Corps, his

account of post-1920 events is sketchy and superficial.

With regard to the Ebert government's controversial role in the formation of the Free Corps, Koch takes the position that, given the constraints imposed by domestic political and Allied diplomatic pressures, the government had no choice but to follow the course that it did. Moreover, defeat, revolutionary chaos, and historically conditioned prejudices combined to make it virtually unavoidable that the unexpected alliance of Social Democrats, old military leaders, and newly formed Free Corps would be filled with the misunderstanding and mistrust that led eventually to the Free Corps' alienation from the republic and the army's faulty integration into it. Koch adamantly rejects the idea of a "third way" based on the council movement. Yet one need not accept the councils as a *deus ex machina* to argue that there were alternatives to the military policies adopted in 1918–19. Koch rejects but never systematically refutes the arguments in favor of such alternatives. The result is a somewhat one-dimensional, fatalistic interpretation of events that inhibits substantive discussion of many of the important issues surrounding the Free Corps. A truly analytical, comprehensive history of the latter remains to be written.

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RAINER BÖLLING. *Volksschullehrer und Politik: Der deutsche Lehrerverein, 1918–1933*. (Kritische Studien zur Geschichtswissenschaft, number 32.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 306. DM 48.

The author examines the sociopolitical environment of German public school teachers from the immediate pre-World War I phase to the coming of the Third Reich. In his fluently written monograph, Rainer Bölling devotes much space to organizational history, and in the center of his examination he places the German Teachers' League (*Deutscher Lehrerverein*), which comprised about 62 percent of all organized German primary school teachers in 1907 and 55 percent in 1927. After sketching the situation of teachers before World War I, Bölling moves on to a brief characterization of the main teachers' organizations during the Weimar republican era. An entire chapter is devoted to the sociopolitical and pedagogical objectives of the teachers' league. A further chapter deals with the interrelationship between the league and Weimar political parties. Appropriate attention is paid to the teachers' attitude concerning contemporary school legislation under the jurisdiction of the regional (*Länder*) governments, namely Prussia, and the criti-

cal issue of teacher training. In this latter area Bölling can report some progress for the teachers inasmuch as university norms were introduced in Thuringia into the training process. That this success was short-lived turned out to be one of several disappointments for primary teachers in the republic, who were striving to improve their academic qualifications, their material standard of living, and, consequently, their social status, especially vis-à-vis the highly educated upper school teachers who continued to look down upon their lower-school colleagues. The last chapters of the book are dedicated to the primary school teachers' struggle with adverse conditions during the economic depression at the end of the 1920s and the (resultant) attraction of the Nazi Party.

To this reviewer, the greatest merit of the study lies in the fact that Bölling has authoritatively dispelled the old myth of the German primary teachers' predilection for right-radical ideologies. As the author convincingly shows, the teachers were much more prone to adhere to bourgeois-liberal parties such as the DDP and, further to the left, the SPD. Yet one could argue that, since Bölling has only dealt with the members of the German Teachers' League, he has disregarded anywhere from 30 to 45 percent of the professional group as a whole. He has thus raised the question of the representativeness of his sample. Perhaps it would have been wiser to reverse main and subtitle in this case: this is really a work about the *Lehrerverein* that tells the reader much about "public school teachers and politics" in the period under discussion.

Without question, this book constitutes an important addition to the growing stock of sociohistorically oriented monographs whose task it is to focus on specific (professional) subgroups of German society and to examine in detail their sentiments and patterns of behavior with regard to the various political currents of the day. It is to be hoped that more studies of this nature, concentrating on upper school teachers, lawyers, physicians, and other closely knit interest groups will follow this important volume.

MICHAEL H. KATER
York University

ULRICH SCHÜREN. *Der Volksentscheid zur Fürstenenteignung 1926: Die Vermögensauseinandersetzung mit den deposierten Landesherren als Problem der deutschen Innenpolitik unter besonderer Berücksichtigung der Verhältnisse in Preussen.* (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 64.) Düsseldorf: Droste Verlag. 1978. Pp. 327.

After years of negotiation, costly law suits, and futile attempts at legislation, the disposition of the

property of the former princes remained an unresolved issue for the Weimar Republic. In 1926, the Communists and Social Democrats cosponsored an initiative and referendum on the expropriation of this property without compensation. Despite its popularity, the referendum failed, as did subsequent efforts to resolve these property disputes on the national level, and state governments were left to grapple with the problem. Historians generally interpret the *Volksentscheid* as marginally important, a reflection of a popular mood of negation or a strong republican demonstration without practical results. Only East German historians, for whom the referendum's widespread support represents a successful example of KPD united front tactics, view it as a particularly significant event. While Ulrich Schüren discounts the latter interpretation, he contends that the *Volksentscheid* was of greater consequence than previously recognized. He argues that in Prussia, the state confronted with the major Hohenzollern claims, the referendum was crucial in forcing a compromise settlement. After the referendum the Hohenzollerns and the German National Party in Prussia became more conciliatory; without this change of attitude the Prussian negotiations would have remained deadlocked. To Schüren this indicates that the procedures for direct democracy were more than "nice decorations on the republican constitution" and that initiatives and referendums provided a viable means through which the people could exert direct political influence.

The popularity of the referendum, Schüren explains, was due to "social grievances" and not KPD united front tactics. Ruined by the great inflation, faced with the economic insecurities of 1926, and incensed by potential lucrative compensation for the princes, many bourgeois voters joined the working classes in the demand for expropriation. But any hope of lasting cooperation between these diverse social forces, or even between the KPD and SPD, was an illusion harbored solely by the Communists. The SPD cosponsored the initiative and referendum only because it feared losing constituents to the KPD. Throughout the campaign, the SPD shunned the very notion of an alliance with the KPD and conducted independent political activity; always ambivalent, the SPD preferred a compromise law until the very end. KPD tactics were often counterproductive because they allowed the opposition to brand the referendum as a Bolshevik assault on private property.

Aside from Schüren's analysis of the referendum's impact on the Prussian compromise, little in his account is actually surprising. His book reads like a typical case study of Weimar party politics in which the reactions of various parties and factions are quite predictable. Schüren also seems reluctant to make generalizations that might widen the scope of

his study. Although he refers to the general phenomenon of direct democracy, for example, he does not actually develop this theme nor relate the 1926 *Volksentscheid* to other Weimar initiatives or referendums. Thus it is difficult for readers to maintain their interest through the lengthy descriptions of internal party debates and fruitless negotiations leading up to the referendum.

As a comprehensive study of this rather specialized subject, however, Schüren's work is a very competent piece of scholarship. The book is well organized, clearly written, and based upon extensive archival research and the more important secondary literature. Schüren's study is the most detailed and reliable history of the 1926 *Volksentscheid* to date.

JOSEPH W. BENDERSKY
Virginia Commonwealth University

KARL HOLL, editor. *Wirtschaftskrise und liberale Demokratie: Das Ende der Weimarer Republik und die gegenwärtige Situation*. Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 152. DM 15.80.

Economic crises constitute a severe challenge to the survival of the "liberal state," argues Karl Holl in his introduction, and many would agree. The specter of Weimar is omnipresent again, and the turbulent years of the world economic crisis still evoke enough fears among participants and non-participants alike to stimulate an apprehensive look at present and past conditions. It is quite reassuring, though, that all the contributors to this small volume of essays assure us in good historical manner that the particular circumstances in Germany were quite unique and that—as the only nonhistorian, H.-J. Rüstow, points out—the basic economic problems of the 1930s are solved by now. That may very well be. The historians themselves still struggle with finding out what the main problems in the world economic crisis and in Germany actually were.

The essays presented—on Brüning's policies by H. Mommsen, the *Staatsverständnis* of the left liberals by J. Hess, the panic of the middle classes by P. Wulf, industrial policies by G. Feldman, and the economic rationale behind work creation by H.-J. Rüstow—confirm first of all some of the worst suspicions about the republic and its politicians. The authors emphasize quite unanimously that none of the social and political groups and individuals under discussion truly cared for the Weimar Republic as a democratic republic. And even those who did (like the tiny group of liberals), did not fully understand the rules of the democratic game. They preferred to think in terms of a "predemocratic" ideology of the state beyond factional interests and of an ideal harmony of society rather than in terms of

conflict and a balance of interests. This perspective has been known quite well and has served as a convenient explanation for the decline of the republic. A more interesting consensus seems to be emerging on another theme, which might actually lead back to some of the above-mentioned fears: the very inability of the Weimar politicians, be they democratic or not, and, for that matter, of industrialists and trade unionists to solve any of the political and economic problems they faced during the crisis. Thus, Brüning not only pursued an authoritarian course, he actually worsened the crisis. The industrialists not only followed their own narrow self-interests—which after all might be expected—but also drove themselves and others even deeper into the crisis. Thus, the main conclusion of this book may be that the failures of the authoritarian policies as much as those of the democratic ones ran down the republic and made it an easy prey for the radical solutions of the National Socialists. The failure to perceive and understand the political, economic, and social issues at stake from an authoritarian vantage point seems to be at least as important as the peculiarly antidemocratic ideologies in Germany. One might conclude that the economic crisis showed more clearly the demise of authoritarian concepts than of the barely existent liberal democratic ones.

MICHAEL GEYER
University of Michigan

EDWARD W. BENNETT. *German Rearmament and the West, 1932–1933*. Princeton: Princeton University Press. 1979. Pp. xv, 569. \$35.00.

One of the more puzzling problems in the interwar period of diplomacy is why the Western democracies were willing to accommodate the anti-democratic governments of Germany in the 1930s when they refused similar concessions to the democratic governments of the 1920s. Edward W. Bennett's microscopic account of the rearmament negotiations from 1932 to the end of 1933 provides a complex answer. Bennett starts his study by stating his three-fold purpose: to assess German military intentions and plans; to evaluate Western knowledge of these plans; and, finally, to describe Western diplomatic responses. The organization of the book adheres to this pattern with most of the emphasis on the diplomatic maneuvers of the major powers to accommodate Germany's push for arms equality.

In assessing German military plans, Bennett detects a growing militarism in Weimar; military ends unduly determined, influenced, or closed out options available to the civil government. The German military sought to restore the nation's strength not only to revise the peace settlement but also to

prepare for the day when Germany could seize the initiative in international politics. Needless to say, this concept of an aggressive military preparedness included an array of antidemocratic ideas, including the destruction of socialism, suppression of dissent, and alterations of the structure of Weimar democracy. Unlike most German writers, Bennett argues that military considerations were "possibly the largest consideration" (p. 301) in bringing about the replacement of governments from Hermann Müller through Hitler. It was not that the German military was so keen on Hitler; it was more that he seemed like the best possible choice given a limited number of possibilities. The catch, according to Bennett, was that the military had done so much to limit these options.

British and French awareness of German military plans was quite good. The French especially had accurate, hard intelligence on the Germans, but both misjudged German intentions. The British thought the Germans wanted a small professional army along Seecktian lines that would be cheaper and hence more desirable, while the French thought the Germans were aiming for a mass militia army lead by a trained cadre.

The heart of the book deals with the author's masterful description of how the Western nations started down the road to appeasement with the issue of rearmament. The French were unwilling to fight, but they were willing to support a firm line on the Germans; their Anglo-American friends were less resolute. Bennett comes down hard on U.S. policy, which "bears more responsibility for smoothing the path for Hitler than has been generally recognized" (p. 510). He is equally as hard on the British, for they, too, erroneously rejected the principle of balance of power and permitted German rearmament. In the end the Germans won by default. The dialogue of the deaf, as the French phrased it, was over. The disarmament conference collapsed and with it went collective security and the league.

This is a superb book on a very narrow topic. Bennett's exhaustive examination of the sources, coupled with his usual clearly organized and written presentation, means that this study will remain the standard for a good many years. My major criticism is of the accent rather than the details of the study. Bennett's case for a marked upswing in influence by the German military at the end of Weimar is strong, but I am not completely convinced. Other recent studies argue that the military was not as responsible for the collapse of the republic as Bennett would have us believe. My second criticism of accent is that in his telling of the tale Bennett sometimes overlooks how important the depression was on international diplomacy. The depression mesmerized the leaders of the early 1930s, and no matter how important German rearmament later be-

came, it was not that important at the time. This does not come across as forcefully as it should in the book, especially considering the author's obvious mastery of the topic. But these are differences over accent or nuance; the study is a remarkable piece of work.

EDWARD L. HOMZE
*University of Nebraska,
Lincoln*

WILLIAM CARR. *Hitler: A Study in Personality and Politics*. New York: St. Martin's Press. 1979. Pp. x, 200. \$18.50.

The author intends his book, as he writes in his preface, to be "a contribution to the continuing debate about the historical role of Hitler in the National Socialist era"; his concern, "in a sentence, is with the interrelationship between the personality of Hitler and those social forces which made National Socialism possible." This goal of placing his subject in the context of his time is, as the author says, shared by most biographers and, in the case of a man who, like Hitler, has long been a center of historical scrutiny, is so modest that it would be difficult not to achieve. Hitler the politician, the dictator, and the military commander is portrayed; his intellectual world and his ailments, psychic and somatic, are explored and commented upon. The author is sedulously fair in assessing the manifold possibilities behind Hitler's decisions and performance—so fair that he leaves little opportunity for answering his rhetorical questions with anything but resigned acquiescence. For example, he asks in connection with the crisis that led Gregor Strasser to leave the party in 1932, "is it not conceivable that Hitler exploded with rage and frustration precisely because he was beginning to wonder whether Strasser might not be right?" (p. 35). Or discussing Hitler's anti-Semitism he asks, "can one automatically assume that hatred of the Jews was at all times the dominant strand in his political theory between 1933 and 1945?" (p. 126). Or again, "might it not be that when Hitler singled out the Jews as scapegoats for all the ills troubling Germany he was simply projecting onto them the guilt complex felt by most Germans?" (p. 161). The author's conjectures are certainly possible ones, but putting his questions in such a catch-all fashion makes answering as easy as asking them.

While most historians would doubtless agree with William Carr's premises, this is not true of some of his observations. He calls Ribbentrop, for example, "able but sycophantic." This is an overgenerous judgment of a man of conspicuous ineptitude. Ribbentrop's preparation for the foreign ministry came by way of doing odd jobs in Canada, selling liquor

as well as champagne from his father-in-law's firm, and supplying Hitler with news items taken from the foreign press. Even his wife was surprised when he was appointed foreign minister. So little did he comprehend the world outside Nazi Germany that after the war he wrote a groveling letter addressed to "Vincent" Churchill in which he referred to Hitler as a "great idealist," offering to send Churchill a copy of Hitler's political testament because, as Ribbentrop wrote, "I could imagine that its contents might be adapted to heal wounds . . . [and] in the perilous epoch of our world . . . bring about a better future for all people."

What the author has succeeded in doing is to present his own version of leading theories and speculations about Hitler, but, especially in his psychological section, he leaves the readers to make their own way among them.

EUGENE DAVIDSON
Santa Barbara, California

CHRISTOPHER R. BROWNING. *The Final Solution and the German Foreign Office: A Study of Referat D III of Abteilung Deutschland, 1940-43*. New York: Holmes and Meier. 1978. Pp. 276. \$21.50.

Among the German bureaucrats who took part in destroying European Jewry were members of the Foreign Office. Christopher R. Browning's book is intended as a case study of such civil service personnel, their individual motivation, and their role in the evolution of policy leading to genocide. He examines principally Undersecretary Martin Luther, von Ribbentrop's ambitious deputy in nazifying the ministry, and some half-dozen subordinates of the "Jewish desk" in the "Division Germany" established for that purpose. Whereas Luther was an "amoral technician of power" (p. 28), ruthlessly carving out a political empire for himself within the Nazi hierarchy, his "Jewish experts"—"a random cross-section of German bureaucracy"—were efficiently meticulous careerists who routinely performed the inhuman tasks assigned to them. Neither ideological fanaticism nor blind obedience motivated these "banal bureaucrats"; rather, they sought to preserve untarnished their reputations for reliability. Their sole error, one stated at a subsequent trial, was in not keeping their names off documents wherever possible! (pp. 178 ff). Precisely that failure enables the author to trace in detail "who initiated what" in implementing the Final Solution (or rather solutions), when and by whom information on each phase—from forced emigration through physical extermination—was known within the Foreign Office, and, especially, what "input" the office then provided in the expeditious res-

olution of the "Jewish problem." Its increasingly overriding significance among Hitler's aims meant that prewar marginal involvement gave way to the zealous initiation of measures (for example, regarding the deportation of stateless Jews within German-controlled territories) by these desk-bound killers in order to preserve the office's jurisdictional position vis-à-vis other participating agencies, notably the SS. The patchwork pattern of wartime Nazi international relationships and changing German military fortunes necessarily limited and varied opportunities for "diplomatic arm-twisting" (pp. 125 ff) where such was needed. Within these bounds, "Division Germany" all too successfully helped facilitate the "frictionless operation" of the machinery of destruction.

At least in its broad outlines and country-by-country development, much of this story was already known; nor do Browning's interpretations of persons and events generally depart from those of previous writers. (One exception is his favorable view of Luther's contribution to postponing a *judenrein* Denmark, pages 250-51). Furthermore, his self-imposed focus upon a single department of the Foreign Office, albeit the key one, sometimes obscures the much wider participation of German diplomats—both high-ranking "conservatives," such as State Secretary von Weizsäcker (pp. 72, 79, 95, and so forth), and ambassadors like Mackensen in Rome—in promoting Nazi goals, evidence for which his account amply provides. Only one, Wilhelm Melchers of the Near East desk (Political Division), displayed "courage, invention and determination" in saving lives, in particular Turkish Jews abroad who were initially unprotected by their government and marked for internment and "resettlement" (p. 156). Among the nations, aside of course from the Danes, the Italians alone systematically and effectively sabotaged Nazi plans wherever they could: "in gratitude for Italian protection, the Jews of Nice [in 1943] . . . raised . . . three million francs to aid Italian victims of Anglo-American air raids" (p. 167). Browning's contribution emphasizes again that the resolve to murder far outstripped that to rescue Nazism's victims.

LAWRENCE D. STOKES
Dalhousie University

MANFRED OVERESCH. *Gesamtdeutsche Illusion und westdeutsche Realität: Von den Vorbereitungen für einen deutschen Friedensvertrag zur Gründung des Auswärtigen Amtes der Bundesrepublik Deutschland, 1946-1949/51*. Düsseldorf: Droste Verlag. 1978. Pp. 204. DM 48.

After Germany's collapse in 1945 the Allies divided the country into four occupation zones. By the end

of 1946, after the Council of Foreign Ministers had negotiated the minor peace treaties and before the zonal division of Germany had changed appreciably, the Allies were ready to take up the German peace treaty at a meeting scheduled to begin in Moscow in March, 1947. At about the same time, certain Germans—among them numerous former foreign service officers who had found their way into private research institutes and the local governments established by the occupation powers—tried to create an agency that could function in the absence of a foreign office, which had gone out of existence in May 1945. They were motivated by various things: the prospect of a peace conference without German participation, hopes that Germans might eventually be invited to participate, and the mushroomlike appearance of uncoordinated and often contradictory public statements on Germany's future by various German writers and officials. The book under review is a study of these people and their efforts.

Based on research in official records and personal papers, enriched by interviews and memoirs, the book is an original contribution. The emphasis is on the German Office of Peace Questions (*Deutsches Büro für Friedensfragen*), its history, and its eventual assimilation into the Foreign Office of the Federal Republic of Germany. Manfred Overesch is at his best on how the question of a "national representation" affected the German Office of Peace Questions. One group believed that the minister presidents of the *Länder* (states) should represent Germany to the outside world, the other that the political party leaders were the true spokesmen for the German people in the absence of an elected central government. The former were strongest in the American zone, where they drew support from Bavaria; the latter in the British zone, where they drew support from Kurt Schumacher and the Social Democrats. Unable to resolve their differences on who should control and instruct the proposed national Office of Peace Questions, the Germans failed to establish even a bizonal office, much less one for the four zones.

Despite its originality, the book is in some respects a disappointment. It is weak on the larger context of the Allied occupation, and it is burdened with much undigested detail on discussions about structure, organization, and jurisdiction. There is little about what the Germans proposed to do or say if they were to participate in the peacemaking. What did they want regarding the Saar? The Ruhr and Rhineland? Reparations? Were they prepared to give up Prussia without a struggle, if they could keep their western boundary?

Overesch accepts Wilhelm Kaisen's inaccurate chronology of the minister presidents' conferences

(p. 93), his English includes "sekret" and "Moscou," and his German includes "gehandikapt," "favorisierte," and "minimalisieren."

JOHN GIMBEL
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FRIEDRICH RENNHOFFER. *Ignaz Seipel, Mensch und Staatsmann: Eine biographische Dokumentation*. (Zeitgeschichtliche Bibliothek, number 2.) Vienna: Hermann Böhlaus Nachf. 1978. Pp. x, 800. DM 125.

It is in the nature of historical writing that there should rarely be a definitive book, particularly in the field of contemporary history: the arguments go on, prompted by the availability of new sources and, of course, by the changing perspectives of the historians. Although this reviewer put Ignaz Seipel on the map, so to speak, with a carefully documented "topical biography," which addressed itself to Seipel as a distinguished but controversial Austrian and, indeed, European statesman of the interwar period, he could not expect his work to be definitive. Responses from the Catholic as well as the Marxist side were inevitable; more important, someone might yet uncover the hitherto-missing papers of the Christian Social Party of the 1920s and 1930s.

The book under review fails altogether to take cognizance of the previous literature on Seipel, including the brilliant though quirky work by Ernst K. Winter. The questions for the reviewer and reader are, then, whether or not the discipline of history is so cumulative as to require the historian to consider previous research and interpretations and whether or not the volume under review has enough new materials and perspectives to justify its publication in the scholarly vacuum in which it clearly maneuvers.

Friedrich Rennhofer's new biography of Ignaz Seipel is a labor of love more than of scholarship. The new documentation on which it claims to be based is minimal. Moreover, some vital political materials have not been consulted: those of the official Austrian archives only spottily; those of the German Foreign Office not at all; the party archive, alas, has not been found. Furthermore the eight-hundred-page narrative is devoid of any attempt at analysis and interpretation. A study of Seipel simply cannot avoid, as this one does, dealing with the issues that make Seipel a statesman of distinction and importance: Seipel as a representative of political Catholicism confronted with the problem of accommodation to modernity and as a postimperial figure in a world of independent "successor states" jockeying for one or the other pattern of Central European order. Seipel's positions on monarchy, re-

public, democracy, the corporative state, National Socialism and his stand on Austria's independence, neutrality, the Anschluss, and Danubian confederation are all issues that must be faced head-on by anyone aspiring to make a scholarly contribution to twentieth-century Central European history with the focus on Seipel.

A mere "biographical documentation," which the volume under review aspires to be, is not enough, in particular since Seipel as a private person and as a mind would barely justify a major historiographical effort. Only the political intent elevated Seipel above the commonplace and the parochial. Nor is hagiography, the genre to which this volume in fact belongs, enough, since Seipel, far from being a saintly personage, made his mark in history by an essentially political vision and by his particular way of dealing with the temptations of political power.

The absence of analysis and critical approach is manifest all over this book. A case in point is the treatment of Seipel's wartime work, *Nation und Staat*, whose contents are taken for gospel by the author rather than analyzed for what they are: a poor man's Lord Acton and a poor man's Meinecke (both of whom Seipel, in fact, seems to have been unaware of) and also a political document—a significant critique of the modern nation state from the perspective of the Habsburg supranational idea and, moreover, a striking critique of the Ausgleich and its consequences.

Was the socialists' epithet for Seipel, the "prelate without mercy," merely "a dangerous slogan based upon some few words taken out of context" (p. 514)? And can Seipel's concept of "true democracy" be taken on face value? Does it not require semantic scrutiny that leads away from what is commonly understood by democracy? After all, at about the time that Seipel was lecturing about "true democracy," he engaged in a close exchange of ideas with none other than the chief theoretician of Catholic corporatism in Austria, Othmar Spann (whose mention is strangely altogether omitted in Rennhofer's book), and also came into close contact with the Heimwehr. Although this evidence does not make Seipel an "architect of counter-revolution," it certainly suggests a disillusionment on his part with the workings of parliamentary democracy and the failure of the policy of accommodation.

We have cause to be grateful to Rennhofer for having added to our knowledge of Seipel's day-by-day movements; but to the argument concerning Seipel's statesmanship and historical stature this volume has contributed close to nothing.

KLEMENS VON KLEMPERER
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GERHARD BOTZ. *Wien vom "Anschluss" zum Krieg: Nationalsozialistische Machtübernahme und politisch-soziale Umgestaltung am Beispiel der Stadt Wien 1938/39*. Introduction by KARL R. STADLER. Vienna: Jugend und Volk. 1978. Pp. 646.

As his choice of title already suggests, Gerhard Botz pursues a twofold objective, offering a history of Vienna under National Socialism until the outbreak of the war and analyzing National Socialist policy to illuminate through the concrete Austrian example the methods of government by the Third Reich. The book is a very detailed and competent study of developments in Vienna during a turbulent period, a study that leaves out no important aspect of life in the former Austrian capital. The author gives special attention to the seizure of power from "below," from "above, and from the outside," to the plebiscite of April 10, 1938, and to the early attempts to win over the Catholic Church, the working masses, and other groups; he also focuses on Nazi policy toward the bureaucracy, the Jews, and the churches and on the growing opposition to the Third Reich and the annexation. In spite of the apparent Austrian mass consent or acquiescence to the *fait accompli* of the Anschluss, Botz does not look upon the 1938 union as a voluntary link-up, such as many Austrians had previously desired. The author holds that in the interwar period the relevancy of the idea of union and of the movement toward Anschluss was at times indisputable. But the total dissolution of Austria and the extinction of its very name during the National Socialist domination of the country, in combination with repression and terror, became the catalyst for the self-discovery of Austrian nationalism and the development of Austrian state-consciousness and led to the creation of that minimal consent among former political opponents about democratic rules without which the country could not have experienced a rebirth. Botz freely admits the initial enthusiasm of many Austrians for the Anschluss, the widespread apathy of the population vis-à-vis the dictatorial methods of Nazi politics, and the absence of early political resistance—all matters about which thoughtful Austrians still display some uneasiness. But soon, he asserts, National Socialism lost this early mass consent.

This study is unquestionably the fullest treatment of the period in any language. The work of a younger Austrian historian, it is marked by a high degree of objectivity and strength of democratic convictions; Botz does not refrain from voicing his opinion about the basic "inhumanity" in theory and practice of the NSDAP and about its leaders and activists, though his purpose is to explain rather than to condemn. Equally important, there is no

trace here of the *grossdeutsch* infatuation that gripped not only German but also Austrian historiography of the interwar period and even survived the birth of the Second Austrian Republic.

The book should have included a short treatment at least of how the rest of Austria fared under the prewar Nazi occupation. Also, due to the structure of the study, some minor repetitions have apparently proved unavoidable: the author returns frequently to themes with which he has already dealt. Although there is little doubt that even during the limited time of a year and a half Nazi policy in Austria underwent some modifications in emphasis, methods, and tone, the basic objectives of National Socialism in most areas hardly underwent a radical change. Finally, an introductory account of the Anschluss movement, 1918–38, using recently published studies (such as those listed in footnote 13, page 562, but not in the bibliography) as well as some other books dealing with the First Austrian Republic, would have provided a needed historic perspective. But Botz's writing is lucid, his analysis penetrating if not always original, and his impartiality exemplary. Although he is preoccupied with the intraparty struggle of the leading Nazis in Austria—with the “institutional anarchy” of Nazi politics that left Hitler in the important role of “arbitrator”—he does not neglect social and economic problems of Austria's development.

Since the study focuses on “the relatively most successful and the least inhuman phase” of the domination of German fascism in Vienna, the author tries to balance a perhaps false impression the reader might gain by letting Karl R. Stadler write an introduction “Provinzstadt im Dritten Reich,” which sketches the fate of the city until 1945, dwells on Hitler's ambivalent though always hate-filled attitude toward Vienna, and summarizes the permanent human and material losses suffered by all of Austria. Stadler recalls that, as consequence of seven years of Nazi occupation, altogether half a million Austrians lost their lives: some died in concentration camps and Gestapo jails; fifty-one thousand five hundred Jews were deported and annihilated; and three hundred and eighty thousand Austrians never returned from the battlefield.

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PIERRE CASPARD: *La Fabrique-Neuve de Cortaillod: Entreprise et profit pendant la Révolution industrielle, 1752–1854*. (“Recherches,” number 29.) Paris: Publications de la Sorbonne. 1979. Pp. 227. 95 fr.

This monograph analyzes a Swiss cotton cloth (calico) printing firm located in Cortaillod, near the

city of Neuchâtel. Pierre Caspard has found rich archival materials in the Neuchâtel State Archives, where the company's records now rest, and in other local repositories. He also has used a surprisingly abundant secondary literature on this industry (*indienne*) in Western Europe. Caspard suggests that the century-long history of the Fabrique-Neuve illustrates the growth of a leading industry in the later eighteenth century, that it reveals how a large manufacturing enterprise operated in this period, and that it shows the traditional vitality of Swiss capitalism, always seeking opportunities for its skilled labor and capital.

In the early eighteenth century the over twenty-five thousand inhabitants of the principality of Neuchâtel, then a dependency of Prussia, lived largely from agriculture, but in the towns and villages a vigorous commerce flourished, based on the export of wine and import of manufactures and foodstuffs. Caspard argues that the economic elite, disgruntled by the low rate of return on farm land, sought to develop manufacturing in order to draw more people into the area, which would increase demand for farm produce and consequently raise prices and profits, and to find a more remunerative outlet for capital. Two new industries had already appeared in the principality in the late seventeenth century: lacemaking and watchmaking. Calico printing arrived just after 1700, favored by prohibitions on its manufacture and sale in France and England. In common with the other new activities, it dealt with a product of low bulk but high value. This business developed slowly at first, employing but two hundred and fifty persons in the principality by mid-century. At this point Claude-Adam Dupasquier established the Fabrique-Neuve, which began printing calico in 1752. Four generations of his family operated the factory for a century. A peculiarity of this firm was that for most of its life it engaged only in manufacturing. A wholesale house in the city of Neuchâtel obtained the cotton cloth, contracted with the Fabrique-Neuve to print it, and then sold the product. Close financial and personal ties existed between the manufacturing and selling firms, and in 1818 they finally merged.

In several interesting chapters Caspard analyzes the operations of the factory, discussing its capital, the evolution of production, costs and selling prices, and profits. He includes many tables, charts, and maps as well as photographs of some of the buildings that still remain. The calico printing industry in the region employed a peak of some two thousand workers in the 1780s. The apogee of the Fabrique-Neuve's success came in the following decade when its output was the largest in Continental Europe, and it employed some six hundred workers. Difficulties ensued thereafter, first the commercial

problems occasioned by the Napoleonic Wars and then tariff protectionism in France and elsewhere. Labor costs rose to higher levels than in neighboring countries. The Fabrique-Neuve tried to develop markets overseas but met serious competition from large producers in major countries. The company did not play the card of heavy investment in new technology. Management reluctantly dropped cloth printing in 1854, shifted the factory to watch-making, and then in 1879 turned to cable manufacturing in which it is still engaged. Caspard does very little with social and labor matters in this study but has published his findings on these subjects elsewhere.

His book should be added to that of F. Jequier on the Fleurier Watch Company and the articles of R. Darnton on the Société Typographique de Neuchâtel as another important recent contribution to the fascinating economic history of the principalities and cantons of Neuchâtel.

JAMES M. LAUX
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ERICH GRUNER, editor. *Die Wahlen in den Schweizerischen Nationalrat, 1848–1919: Wahlrecht, Wahlsystem, Wahlbeteiligung; Verhalten von Wählern und Parteien; Wahlthemen und Wahlkämpfe*. Volume 1, *Erster und zweiter Teil*; volume 2, *Anmerkungen*; volume 3, *Tabellen, Grafiken, Karten*. Assisted by GEORGES ANDREY *et al.* (Helvetia Politica. Series A, number 6.) Bern: Francke Verlag. 1978. Pp. 1189; 300; 540.

Erich Gruner's *Elections to the Swiss National Council, 1848–1919* is a monster of a work. The first volume, divided into two separately bound parts, consists of some eleven hundred pages of small print. Volume 2 contains the "footnotes," three hundred pages of them, packed with additional information; the oversized volume 3 has hundreds of tables, charts, graphs, and maps.

The work covers the first seven decades of the modern Swiss state. It begins in 1848, when the new constitution created a federal legislature with two houses, roughly on the American model. The lower house, the National Council, was elected on the basis of a modified majority system according to the number of people living in the Swiss cantons. One never would have suspected that such a harmless sounding statement would necessitate two thousand pages of print to explain all of its ramifications, peculiarities, and meanings. 1919 is a natural ending point for the work since in that year the majority system of electing national councillors gave way to the system of proportionate representation according to party strength. The work is a detailed history of the first decades of the modern Swiss con-

federation as well as a detailed study of a system of representative democracy, probably the most thorough in existence in any language about any country.

Historical approach (the story of what actually happened during the elections in all twenty-five Swiss cantons) and political science methodology (the analysis of underlying issues) are roughly balanced. This dualism marks and at times mars the work. There is little internal cohesion. Chapters usually begin with a review of the literature on the subject, but discussions of pertinent secondary works and sources also turn up frequently in the text and the footnotes. Technical dissertations about computer analyses of statistical data are interspersed with narrative segments. Parts of the work are written in French; a French résumé of the German-language chapters provides a convenient short-cut to the materials.

From 1848 to 1917 there were twenty-four general elections at three-year intervals. The history and analysis of each one of these elections form a major part of the work. The elections to the National Council are seen as a means for an established political system to adapt itself to changing economic and social circumstances. The adaptation took place in small steps across a series of elections and through varying party alliances. The Swiss political culture is characterized as having been able to soften the impact of painful conflicts or to prevent them altogether. Yet elections during this period never degenerated into mere routine acts of voting. They often were almost direct plebiscites on the decisions of the government and closely paralleled referendum balloting.

During two dynamic phases of economic expansion and social change (1849–73 and 1894–1913) the elections helped to bridge differences and balance conflicting interests, largely under the umbrella of the dominant Radical Party. Only when this power of adaptation was lost, due to the pressure of World War I and growing class strife, did the majority system collapse and have to make room for elections under a system of proportionate representation.

It is obviously impossible to give even an overview of the unbelievable wealth of information provided by Gruner and his collaborators. Whether they deal with the constitutional and legal bases of the system, the election process, voter participation, parties, political background of the formation of electoral districts, election themes and struggles, preparation and execution of elections, or peculiarities of Swiss elections under the majority system in plural electoral districts, they always seem to exhaust the subject. Their work is the definitive treatment of an important aspect of recent Swiss history.

Thanks to the expertise and methodological thoroughness of its authors, it will become a model for similar studies in other countries.

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JEAN-CLAUDE HOCQUET. *Le sel et la fortune de Venise*. Volume 1, *Production et monopole*. Lille: Université de Lille III. 1978. Pp. 352.

Venetian commerce was based on salt. Already before the year 1000, the Venetians found salt to be the most profitable export from their lagoons. They brought salt inland to exchange for grain and other necessities. This important book describes the production of salt and the mechanisms of Venetian control over the supply of salt throughout the Venetian empire from the mid-thirteenth to the early seventeenth centuries.

Initially, Jean-Claude Hocquet defines the metrology of salt and describes the techniques and areas of its production in late medieval Mediterranean Europe. The Venetians dealt in sea salt, produced along the Mediterranean and Adriatic, where salt was obtained by the evaporative action of the sun on salt water. Several different qualities of salt were produced from the Istrian peninsula, the coasts of Dalmatia, Albania, Apulia, and the Ionian islands. Venetian ships also brought back different kinds of salt from the Aegean, Syria, Egypt, Libya, Sardinia, and the Balearics.

Venice attempted to monopolize the supply of salt in its trading area, but, as the author admits, in the end the Venetians maintained only a very imperfect monopoly. Venice controlled the supply of Adriatic salt by concentrating production at certain locations along the upper Adriatic. This salt could be carried by sea only to Venice, making Venice the staple port. Those salt-producing areas owned outright by Venetians were forced to return a percentage of the harvest to the Venetian state. Other salt-makers had to sell most of their salt to the Venetian Salt Office at a fixed price. The remainder of the salt was consumed locally or sold at a higher price to muleteers bringing foodstuffs from the hinterland. Venice obtained an alternate supply from its fleets returning from farther voyages. With these imports, Venice became the sole source of supply of salt for the Veneto and Lombardy.

Hocquet demonstrates that the quantity of salt produced in each locale was subject to extreme variations from year to year, month to month, and even week to week. He argues that in the short run this variation was caused by the climate. For the *longue durée*, government policies influenced the supply more than the weather. The Venetian monopoly re-

stricted salt production on the Adriatic during the thirteenth and fourteenth centuries in order to encourage its fleets to bring Mediterranean salt back home. By the sixteenth century, however, the Turkish expansion and the decline of Venetian overseas commerce caused Venice to support once more the production of salt on the Adriatic coast.

The themes and methodology of this fine book are typical of the *Annales* school, to which the author belongs. Twenty years of archival research have produced this monograph with its exhaustive fifty-seven pages of bibliographies, index, table of contents, and three excellent maps. The author promises other volumes on maritime commerce in salt, salt production in Venice itself, and the markets for Venetian salt and its *gabelles*. This book succeeds admirably in extending our knowledge and understanding of Venetian commercial leadership during three hundred and fifty critical years.

LOUISE BUENGER ROBERT
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GIORGIO PORISINI. *Bonifiche e agricoltura nella bassa valle padana (1860-1915)*. (Studi e Ricerche di Storia Economica Italiana nell'Età del Risorgimento.) Milan: Banca Commerciale Italiana. 1978. Pp. 436.

Land reclamation in the lower Po Valley may not sound like a topic likely to generate much controversy. This is precisely what has happened, however, in the context of Italian history where land reclamation in that region is closely tied to developments of national scope. It is a merit of this book that it discusses its narrow topic in a broad context marked by the consolidation of agricultural capitalism and by the evolution of a special relationship between government and private initiative. Another important dimension of the topic, namely, the emergence of an organized agricultural proletariat, is reserved for separate treatment in another volume in this series that is scheduled for publication in the near future. Although the decision to deal with labor separately may leave some readers with an incomplete understanding of the social ramifications of land reclamation, it is probably justified by the complexity of the issues discussed here by Giorgio Porisini and by the vast documentation that he incorporates. Indeed, with more than half the book devoted to statistical tables, we are given what amounts to a small portable archive. The rest is well written, clearly organized, and easy to follow.

Porisini is most interested in the evolving relationship between public and private initiative. The turning point in this relationship occurred in 1876, when the liberal left came to power with a vaguely

defined program of meeting popular needs. Ironically, in the case of land reclamation such a program enabled well-off private investors to reap huge profits by buying at low prices extensive tracts of marshy, underutilized land and reselling the same land at inflated prices once the government had committed itself to reclaiming that land for reasons of public health and economic progress. As the author points out, "the distinguishing trait of Italian legislation in matters of land reclamation was the introduction of hygienic and sanitary considerations as determining and justificatory grounds for extending government support to private property interests" (p. 76). The author's documentation appears to be most convincing precisely when he analyzes the web of private interests that inspired and guided the appropriate legislation through parliament.

More puzzling and less convincing is the author's assessment of the impact of land reclamation on the agriculture of the Po Valley. On this point he wavers between positive and negative views, with the emphasis definitely on the latter. For, while he discusses at length several instances of newly formed enterprises that operated efficiently with the most up-to-date techniques, he also insists that on the whole land reclamation led to the formation of speculative enterprises more interested in short-term profits than long-term productive investments. His conclusion is that extensive public support for private initiative in the form of outright subsidies and tax incentives did not produce sustained economic growth and that sizable private profits were not reinvested in agricultural enterprises. Thus the author reiterates familiar themes of current economic analysis in Italy.

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MEIR MICHAELIS. *Mussolini and the Jews: German-Italian Relations and the Jewish Question in Italy, 1922-1945*. Oxford: Clarendon Press, for the Institute of Jewish Affairs, London. 1978. Pp. xii, 472. \$39.00.

Fascist Italy's "Manifesto of the Race" was promulgated in July 1938. Given Mussolini's earlier barbs directed at the anti-Semitism of "that idiot in Berlin," his conversion to racism comprises one of the trickier conundrums of fascist historiography. Above all, it must be asked how much it was due to the Rome-Berlin Axis. In the book under review the themes of Italian Jewish persecution and Italo-German relations are intertwined—a successful enough design save for a weak chapter on the outbreak of World War II that loses sight of the Jewish question.

The strength of Meir Michaelis's work lies in its careful delineation of Mussolini as a covert anti-Semite from the beginning, albeit one who never degenerated into a rabid executioner. The Duce's ambivalence is ascribed to a desire to mediate between international Jewry (he believed such a force existed) and the genocidal fanatics—rather in the way he aspired to mediate diplomatically between the Western powers and Germany. His goals were moderate anti-Semitism and selective revisionism. Freedom of maneuver on both counts, however, vanished by 1938. The search for a "dynamic ideology" thereafter led straight to an imitation of the Nuremberg Laws; "Mussolini had all sorts of grievances against the Jews but only one reason for persecuting them as a 'race'—his ill-fated alliance with a Jew-baiter" (p. 125). It follows that the Fascist regime's claim (given credence lately by Luigi Preti and Gene Bernardini) to an indigenous racism, nurtured in Ethiopia and based on peoples and nations rather than race, receives short shrift here. Yet, while affirming the "Aryan-Nordic" character of fascist racism, Michaelis is at great pains to circumscribe Nazi Germany's role. German encouragement there certainly was, but Nazi influence was always "indirect." In the final analysis, "the Duce's sudden declaration of war on the Jews" was "unrequested and unexpected" (p. 190).

Fascist Italy's embrace of racism was determined by Mussolini alone. It was not forced on him by Hitler nor by the radical racist wing of the PNF. Interlandi and Preziosi spoke for Mussolini, not he for them. With this contention Michaelis refutes Renzo De Felice, *Storia degli ebrei sotto il fascismo* (1961). Michaelis is also somewhat kinder to the Vatican than De Felice.

Inevitably, *Mussolini and the Jews* invites comparison with De Felice's earlier work. Both books are organized in two chronological sections with the rise of the Axis serving as a watershed, and basically the same narrative ground is covered. But Michaelis's source material is far richer; whereas De Felice relied almost exclusively on Italian documentation, Michaelis has made splendid use of non-Italian archival records recently accessible in Israel, Germany, and Britain. De Felice was a pioneer, moreover, in the controversy over Fascist anti-Semitism; Michaelis, conversely, has profited by taking into account fifteen years of scholarly debate, which lends his own interpretations a sense of judicious consideration. In consequence, *Mussolini and the Jews*, although we may be sure it is not the last word on the subject, affords a comprehensive and sensitive treatment that will be very hard to match.

ALAN CASSELS
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LAJOS DEMÉNY *et al.*, editors. *Răscoala secuilor din 1595–1596: Antecedente, desfășurare și urmări* [The Szeklers' Uprising of 1595–96: Antecedents, Evolution, and Results]. Summary in German. Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 336. 19 L.

In the sixteenth century the spread of the so-called second feudalism in Eastern Europe reached the Magyar-speaking mountain people who lived in the Transylvanian Carpathians. These Szeklers were free villagers who had preserved egalitarian, communal forms of organization from their tribal past and who played an important role as defenders of the Hungarian kingdom's eastern frontier. The Szeklers resisted the change, even as their traditional social forms were weakened by internal differentiation into new classes. A nobility was formed from those who could provide armed horsemen, a large class of commoners was relegated to the ordinary infantry, and the poorest, without draft animals or sufficient land, fell into serfdom.

Transylvania was in a state of endemic war among the Habsburgs, Ottomans, and the virtually independent (after 1526) local princes as well as among the Poles and Vlachs. The Szeklers were a vital military resource for the participants in these conflicts, who tried to win them over as best they could. Domestically, the free Szeklers were fighting to prevent further encroachments on their traditional rights and to curb their nobility. In 1562 there was a large-scale revolt, which was severely repressed by the Transylvanian government. But resistance continued, and there were uprisings in 1595–96. Szekler discontent was cleverly exploited by the Wallachian prince Michael the Brave in the late 1590s, when he briefly conquered Transylvania during his challenge to the Ottoman Empire. He guaranteed the common Szeklers their freedoms; and, even after his death and the return of the Magyar princes, the authorities bowed to the inevitable and allowed this situation to persist. Szekler society was too independent and warlike to be subjugated by a nobility, and it provided too important a resource for the princes of Transylvania to be tampered with.

This first-rate work of Magyar scholarship in Rumania was written by fourteen different authors. It traces not only the diplomatic and international imbroglios of the century but also the social and economic changes among the Szeklers. Much of the material might be put to use by comparative historians studying the survival of similar free, warlike mountain communities throughout Europe, particularly in the Swiss Alps and the Pyrenees. The book also shows that during this period, at least, ethnic particularisms were weaker than they were to be-

come. While the nobles, princes, and emperors shifted alliances and scrambled to increase their revenues and powers, the ordinary Szeklers were just as opportunistic and joined those who promised them the most freedom and security—no matter what their religion or native language.

The reader is given a good sense of why the various actors behaved as they did, what their interests were, and how they tried to satisfy them. A flexible use of Marxist categories is combined with a good dose of common sense and skillful inspection of the documents. The Rumanians occasionally issue translations of their scholarly works, and one can only hope that they choose to do so with this book. It now has a six-page German summary, but this is too brief to help the Western scholar who might not read Rumanian. This piece of scholarship deserves a wider audience.

DANIEL CHIROT
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MIRCEA MUȘAT and ION ARDELEANU. *La vie politique en Roumanie, 1918–1921*. Translated from Rumanian by RADU CREȚEANU. (Bibliotheca Historica Romaniae, Monographies, number 19.) Bucharest: Editura Academiei Republicii Socialiste România. 1978. Pp. 259. 20 L.

The Rumanian original of Mircea Mușat and Ion Ardeleanu's *La vie politique en Roumanie, 1918–1921* was the first of a number of very basic historical studies to appear on twentieth-century Rumania before World War II. (Among other such works are Mircea Iosa and Traian Lungu, *Viața politică în România, 1899–1910* [1977]; Anastasie Iordache, *Viața politică în România, 1910–1914* [1972]; Mihai Ruseanu and I. Saizu, *Viața politică în România, 1922–1928* [1979]; and Emilia and Gavrilă Sonea, *Viața economică și politică a României, 1933–1938* [1978].) The Mușat and Ardeleanu book is the only one to be published so far in a Western language. It is based on Mușat's dissertation of a decade ago, and the present French version is a translation of the greatly revised and expanded second Rumanian edition of 1976.

The book's virtues are a straightforward sorting out of the various political groups and their activities during these three formative years in the development of Greater Rumania. The book's major limitation is an understandable tendency to tread very cautiously with respect to interpretation.

Each chapter of *La vie politique* treats one party, beginning with a short history of the group at hand, proceeding to an interesting analysis of the class affiliation of its leaders and supporters, continuing with an exposition of party ideology, and ending in

a review of party activities during the years 1918–21. All of this is competently, and sometimes quite imaginatively, done. Given that the book is entitled *La vie politique*, however, rather than *Les partis politiques*, one might wish for a separate chapter on King Ferdinand, who played a central part in the political drama of these years, and for a fuller account of his very close working relationship with Liberal Party leader, Ion I. C. Brătianu. Likewise, the study says virtually nothing about the personalities of such key political figures as Brătianu, Iuliu Maniu, Ion Mihalache, and General Alexandru Averescu, although the characters of the political protagonists were often crucial determinants of public policy in interwar Rumania. Nonetheless, a good many questions of fact are clarified and some central issues, deserving further serious research, are raised. Particularly illuminating, for example, are the discussions of the successive reorganizations of the National Liberal Party and of the composition of Averescu's People's League.

The authors make good use of a wide variety of published sources—newspapers, diaries, books, journal articles, and pamphlets—as well as a certain amount of archival and mostly unpublished material, such as the memoirs of Constantin Argetoianu. The bibliographical information provided will certainly prove invaluable for future researchers. A list of cabinet members in successive governments and a chart on parliamentary representation are useful too, though unfortunately there is no index whatsoever.

The publication of *La vie politique en Roumanie, 1918–1921* is a most encouraging development. The book makes a very substantial contribution to our understanding of the workings of interwar Rumanian political parties. It is to be hoped that other books of this sort will also appear soon in translation.

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BIANCA VALOTA CAVALLOTTI. *Nicola Iorga*. (Gli Storici, number 7.) Naples: Guida Editori. 1977. Pp. 312. L. 4,500.

Bianca Valota Cavallotti's bio-bibliographic study of the Rumanian historian and politician Nicolae Iorga is a succinct if somewhat superficial volume, apparently designed to acquaint Italian readers with the prodigious career of a man whose intellectual formation and activities were somehow related to his work and life in Italy.

The author rightly depicts Iorga as a Renaissance figure, as a giant among the historians and in-

tellectuals of late nineteenth- and early twentieth-century Rumania. She also rightly emphasizes the innovative character of his historical and political thought prior to his becoming, early in the twentieth century, a less than innovative exponent of nationalist ideology and, after the establishment of Greater Rumania in 1918, a less than innovative politician and historian of national heroes and nationalist themes. Cavallotti's study is uncritical chiefly because it is largely based on the written work about and oral accounts of Iorga the man and historian and of his place in Rumanian history and historiography by contemporary Rumanian historians and political leaders. Consequently, a somewhat idealized and distorted image of Iorga and of his work emerges from Cavallotti's volume.

Iorga's significance as a historian rests in the imaginative and often incisive interpretations of major historical problems that transcended the narrow confines of traditional, national, Rumanian history. Iorga's brilliant intellect, fertile mind, and great facility in expressing complex ideas orally and on paper allowed him to use historical data for a variety of purposes not necessarily related to rigorous historical scholarship. His early work was only indirectly related to Rumanian history, and it seems fair to say that his *Geschichte des Osmanischen Reiches* (5 vols., 1908–13) was his best scholarly contribution. His later work, mostly connected in one way or another with Rumanian political problems, was gargantuan in size and scope but often lacking in objectivity and clarity. His reputation among Rumanian intellectuals was generally greater than that which he enjoyed among fellow historians, who could find fault with his methodology, inaccuracy of bibliographic references, and generally polemical style and conclusions.

It is noteworthy that Iorga, the man and his work, were in eclipse for many years after World War II until the nationalist tendencies of Rumanian communism became manifest in the late 1950s. The gradual development of a new Iorga cult during the last twenty years is intimately related to contemporary interpretations of the historic legacy of the Rumanians in relation to traditional foreign enemies and to the place of the Rumanians in universal history.

Cavallotti pays scant attention, if any, to these aspects of Iorga's career. Her work is essentially an Italian version of works on Nicolae Iorga by contemporary Rumanian historians that will allow Italian readers to acquire a sense of the stature and significance of Nicolae Iorga in the history and historiography of Rumania, past and present.

STEPHEN FISCHER-GALATI
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DAVID P. DANIEL. *The Historiography of the Reformation in Slovakia*. (Sixteenth Century Bibliography, number 10.) St. Louis: Center for Reformation Research. 1977. Pp. 50. \$2.50.

Despite the unpromising parochialism of its title, this slender pamphlet is surprisingly stimulating to read. Part of its provocativeness lies, of course, in the sheer novelty of its subject. American historians know all too little about the role of the Reformation in Eastern Europe; to be led by a thoughtful scholar through its bibliographical complexities in an area as important as Slovakia is a welcome experience. But more interesting still is the way in which the historiography of Slovak Protestantism or, more properly speaking, Lutheranism seems to mirror the history of the Slovaks in general. If ever there was a people to have been caught up in the multiform political and ideological vagaries of Eastern Europe, it is they. Hungarian expansionism, Habsburg dynastic imperialism, nationalism, fascism, communism—all have had their day in Slovakia (northern Hungary until the twentieth century), and all have developed characteristic historiographical approaches to the Slovak Reformation. Seeking to justify the post-World War I amalgamation of the Czech and Slovak territories, partisans of that cause treated Slovak Protestantism as an offshoot of the earlier Czech Hussite movement. For their part, Slovak autonomists have downplayed the significance of Protestantism in the area in order to deny the existence of that very same relationship that the Czechoslovaks were promoting. Hungarian historians have traditionally viewed Slovak Lutheranism as simply one part of the Reformed Church in Hungary, which was Helvetic in its general orientation. Since the end of World War II, both Czechoslovak and Hungarian writings on the subject have reflected the views of the current regimes and therefore have minimized confessional concerns altogether.

Both in mind and heart David P. Daniel is with those who try to understand the Slovak Reformation as a specifically Slovak phenomenon, although he does not lose sight of works that place the movement within the social, economic, and political context in which it occurred. As a result, he sustains a tone of cool professionalism throughout the essay. The only moment in which his sympathies run away with him occurs when he speaks of the Slovak Lutherans as "the custodians of the Protestant tradition among Czechs and Slovaks" (p. 32). To this reader at least, this distinction is the property of the Czech Brethren, who endured through centuries of persecution never experienced by Slovak followers of the Wittenberg reform.

In general, the materials chosen for comment and

the comments themselves bespeak a scholar thoroughly at home in the field. At only two points are there gaps in Daniel's reading worth mentioning. One appears in his discussion of the Fugger-Thurzó mining ventures in northern Hungary, where one misses any reference to Götz Freiherr von Pölnitz's massive work on the Augsburg banking dynasty. Another is hinted at in the author's confident reference to Rudolf II's "absolutist inclinations." A reading of R. J. W. Evans's *Rudolf II and His World* (1973) might have forced Daniel to judge a bit more cautiously there. These lapses are very minor, however, and in no way do they impair the usefulness of Daniel's work.

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FRANTIŠEK ČERNÝ and LJUBA KLOSOVÁ, editors. *Dějiny českého divadla*. Volume 3, *Činohra, 1848–1918* [History of the Czech Theater. Volume 3, Drama, 1848–1918]. Prague: Academia. 1977. Pp. 657. Kčs. 160.

In this massive, richly illustrated book a team of twelve Czech scholars, headed by co-editor František Černý of Charles University, depicts a living, multifaceted art that inspired individual creativity while serving the cause of national advancement. As the third volume in a major history of Czech theater, this work ranges from the 1848 revolution to the founding of the Czechoslovak state. The theater surveyed is drama (*činohra*), not the musical or opera, which a future volume will handle. Two volumes that covered theater from antiquity to 1848 appeared in 1968–69.

The underlying concept, rather unobtrusively interwoven in the various sections by different hands, is that the Czech stage, while influenced by West European and Russian models, evolved in close conjunction with internal socioeconomic and political developments. This is hardly a startling hypothesis. In mid-nineteenth-century Austria, theater suffered the constraints of private patronage and official censorship. During the constitutional awakening of the 1860s the assertive Czech middle classes and intelligentsia began to underwrite the stage and to review plays in the press. An urban phenomenon, theater flourished in large cities such as Prague and Brno but also had a small-town following. The sons and daughters of the revolutionary generation entered the profession as the actors, scenic designers, and producers who brought ideas to realization. Indeed, the professionalization of the art and the specialization of its skills, significant features of the era, come across graphically in these pages.

After the 1860s theater had to compete increasingly with other forms of entertainment, eventually and most potently cinema; but it always held a special place in Czech hearts, as witness the patriotic campaign to complete the National Theater building in Prague (1868–83). The authors skillfully trace Czech dramatic motifs from their mid-century romantic phase through the realism and naturalism of the 1890s and symbolism and expressionism before World War I. The powerful links between Czech culture and avant-garde Western trends are apparent. A valuable contribution of the work is to show how emigrant Czech actors practiced the native art in such diverse locations as Vienna, St. Petersburg, and Chicago. We are informed that cabaret, puppetry, and chanteuse, modes in which Czechs were second to none, were tinged from their inception with antiestablishment sentiments, as was the “theater of commitment” (*angažovanost divadla*), which appealed to socially critical audiences.

In their conclusion, the authors note some distinctive characteristics of Czech theater: a concrete portrayal of people in realistic circumstances, an effort to reach viewers not through philosophical abstractions but via satire and parody, and the influence exerted on the actual performances by sovereign directors such as Josef Kajetán Tyl and Jaroslav Kvapil.

This book is the result of herculean research and expert synthesis. It assumes that the reader already knows the dramatic plots, the playwrights' careers, and the controversies that animated critics and aestheticians. Those who lack such knowledge would do well to approach the subject first through the more elementary *České umění dramatické*, (2 vols., 1941) or a comparable work. There is no bibliography, but references from Czech and world literature are provided in text and notes, and the two indexes are comprehensive and exact. Over 360 illustrations, many in color, with captions in Czech, English, and German, delight the eye.

This imposing book immerses us in the original and abundant dramatic art shaped by the Czech people in their nation-building era. One awaits with anticipation other volumes in the series.

STANLEY B. WINTERS

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BRUCE M. GARVER. *The Young Czech Party 1874–1901 and the Emergence of a Multi-Party System*. (Yale Historical Publications, Miscellany, number 111.) New Haven: Yale University Press. 1978. Pp. xv, 568. \$20.00.

The study of Czech history in the Dualist era has attracted a great deal of attention in Czech-

oslovakia. Most of the writings, however, have emphasized the remarkable achievements in the cultural and economic spheres. Insofar as they have dealt with Czech political history, political parties, and strivings, they have tended to view them from the perspective of the T. G. Masaryk-led struggle for national independence during the First World War and the establishment of the Czechoslovak Republic. Not much interest has been shown in those political parties, such as the Young Czechs, who guided Czech politics under Dualism, but aimed, virtually until the end, to reform and not to destroy the Habsburg Monarchy. In the West few monographic studies have been devoted to Czech history of this period, and works on the Dual Monarchy have generally viewed Czech politics from the perspective of imperial interests. In any event, these works as well as the general surveys of Czechoslovak history have paid only scant attention to the Czech political system and parties.

Bruce M. Garver's book provides the first comprehensive examination of the Young Czech (National Liberal) Party, a leading force in Czech politics from its founding in 1874 until the First World War. For the most part, the work is organized topically: the first two chapters analyze respectively the national and the Habsburg contexts of Czech politics; the following three are devoted to the founding of the party, its institutional bases in Czech society, and its rise to the leading position in Czech politics by 1891. The study concentrates on the 1890s, and four chapters detail and discuss the predominant party's behavior toward the crises of this critical decade in the history of Austria-Hungary. The last chapter examines the decline and disintegration of the Young Czech party, the rise of successor and rival parties, and, thus, the democratization of Czech politics.

Throughout the discussion Garver stresses the party's importance and its “constructive” role in Czech and Cisleithanian political life. He does not see the Young Czechs as “radical troublemakers,” as they are usually perceived in Western works on the Dual Monarchy, but rather as pragmatic and responsible politicians, as “able political technicians and formidable political polemicists” (p. 318). He emphasizes the party's contribution to the cause of liberalism and anticlericalism and its defense of civil and national liberties. Undoubtedly, the party made a lasting imprint on Czech politics and “its important place in Czech and Cisleithanian history remains secure” (p. 319). Garver leaves one, however, with the impression that he has been a bit too positive and uncritical in his overall evaluation of the legacy of this “elite party of notables.” He touches upon, but does not give equal weight to, the party's many conceptual and tactical shortcomings,

failings, and, in most respects, very limited success, all of which help explain why "few Czechs have ever held the Young Czech party in great affection or esteem" (p. 319).

Although Garver focuses on the Young Czechs, his lengthy work is not merely a history of a party or of a party system. His scope is much wider. He has drawn extensively on the many specialized Czech scholarly works, especially by Marxist historians, and has also produced a very fine survey of Czech social and political development in the second half of the century. All in all, Garver's clearly written book, which is based on an exhaustive investigation of archival and printed primary and secondary sources, makes a major contribution to our understanding of later nineteenth-century politics in the Czech lands and in Cisleithania. Students of both will await with interest Garver's separate work on the period from 1901 to 1914.

ANDREW ROSSOS
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DAVID W. PAUL. *The Cultural Limits of Revolutionary Politics: Change and Continuity in Socialist Czechoslovakia*. (East European Monographs, number 48.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press. 1979. Pp. x, 361. \$18.50.

Historians read more than they write, but because of their pedantic proclivities their labors often give birth only to pretentious minutiae. Political scientists seem to write a lot more than they read and produce confident but ill-informed generalizations. David W. Paul belongs to that rare species, a political scientist endowed with an abiding sense of history. To judge from the present volume, he reads prodigiously and his generalizations are splendidly informed. He tells the interested reader almost everything he or she ever wanted to know about Czechoslovakia's political culture. The thread that runs through his work is that the history of the Czechs and Slovaks reveals enduring patterns of political culture that even the not-so-gentle Communist masters, lodged in power since 1948, have been unable to obliterate. One of the dominant traits in this culture, according to the author, is political pluralism; the experience of the Dubček interlude of 1968 shows how persistent the pluralist impulses have been.

In academic writing on Czechoslovakia, the Czechs have tended to steal the show: the Slovaks have usually been lumped together with the Czechs without getting their own billing; at best they have appeared as a third-rate appendage to the Czechs. Paul performs something of a tour de force by giving the Slovaks a distinctive profile. In this he has had few precedents in the historical literature on

which to build; because he has to break new ground, some hesitations and ambiguities still remain. In the main, Paul does not quite capture the influence and the dynamism of the Slovak Catholic movement of the 1930s and early 1940s. His observation that the Slovak (Catholic) People's Party remained a "distinct minority" is, strictly speaking, correct but, on close inspection, proves not very helpful. First, that party received approximately 39 percent of Slovak votes in 1929. This may not be a majority, but the figure acquires its proper significance if one realizes that practically nowhere in Europe at this time did a political party win this degree of support from its potential constituency. Even Hitler had to be content with 37 percent of the popular vote at the height of his popularity in Germany in 1932. Moreover, the Slovak People's Party was unquestionably the largest Slovak party, with the next largest party, the Slovak Agrarians, trailing behind with about 26 percent of the vote. This position gave the Slovak People's Party particular dynamism and force that went considerably beyond the numerical support it received at the polls. It was, in fact, very much a party of the political mainstream, and, to this reviewer at least, Paul does not quite confront the implications of this fact. Similarly, he mentions but does not really reflect upon the experience of the independent Slovak state of 1939-45. The Slovak state heightened the national awareness of the Slovak people and in that sense constituted an important stage in the development of Slovak nationalism. That this state was at the same time governed along the lines of Catholic authoritarianism only underscores the difficulty that confronts those wishing to draw up the balance sheet of this experience. How does all this fit into the Slovak political culture?

In the penultimate chapter the author makes an imaginative and original attempt to explore what he calls the "pathos of political non-violence." Few will quarrel with his conclusion that nonviolence has been characteristic of Czech and Slovak history during the last five centuries. One might add that this sets the two peoples apart from most of the nations of Eastern Europe, with the possible exception of the Slovenes.

Whether nations have souls or merely social and political characteristics, Paul's volume will make it possible for us to understand better these aspects of Czech and Slovak history. His work should serve as an invitation to others to attempt such a political profile for the other nations of Eastern Europe.

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HANS HENNING HAHN. *Aussenpolitik in der Emigration: Die Exildiplomatie Adam Jerzy Czartoryskis, 1830-1840*.

(Studien zur Geschichte des Neunzehnten Jahrhunderts, number 10.) Munich: R. Oldenbourg Verlag. 1978. Pp. 316. DM 67.50.

This book is an important addition to the growing literature on the multifarious diplomatic endeavors of Adam Jerzy Czartoryski. Originally prepared as a doctoral dissertation at the University of Cologne, Hans Henning Hahn's study of the Polish prince's diplomacy in the 1830s complements the study published two years earlier by the Warsaw historian Jerzy Skowronek, who treated the subsequent, largely Balkan phase of the activities of the Hôtel Lambert.

Hahn's examination of the first decade of Czartoryski's diplomacy in exile is essentially analytic and topical rather than narrative and chronological. In assessing the aims, programs, and results of the Czartoryski camp, the author also explores the broader issue of the possibilities and limitations of representing a "stateless" nation and the problems of the statesman in exile.

Hahn sees Czartoryski's principal aim in the 1830s as an effort to posit a manifold "polnische Präsenz" (Polish presence)—juridical, diplomatic, and military, no less than national—notwithstanding partition and the setbacks of 1831. Although Western sympathy for Poland's misfortune stopped short of risking war for Poland's restitution, Czartoryski's success in securing British and French support for his interpretation of Russia's violation of the 1815 Vienna accords "furnished the foundation—alternatively the pretext—for the Western powers to meddle regularly in Polish matters whenever it seemed expedient to them" (p. 92).

Czartoryski's hope in the 1830s to establish a unified Polish legion, a military "presence" reminiscent of the one established in the Napoleonic era to further Polish national aspirations, was thwarted not only by the dispersal of the émigrés but also by the desire of several Western states to acquire Polish mercenaries and their reluctance gratuitously to incur St. Petersburg's wrath. More successful, Hahn argues, were the efforts of Czartoryski and his followers to propagandize on behalf of Poland's plight and to foment anti-Russian sentiment in Western public opinion.

Hahn sees the years 1839–40 as a critical turning point in Czartoryski's activities in exile. (The older accounts of Handelsman and Kukiel put little emphasis on this caesura.) The renewed Eastern crisis of this period, which led to France's diplomatic isolation and England's rapprochement of sorts with Russia, prompted Czartoryski to reconsider his exclusive dependence on the Western powers, especially Britain. The failure of short-term initiatives or *faits accomplis*, such as the Vixen incident of 1835,

convinced the prince that a long-range policy was necessary. The belief that a European war leading to a reversal of Poland's status was imminent—a tenet of the 1830s—yielded to the realization that renewed independence would require a fundamental realignment of Europe's balance of power. Accordingly, the prince set about establishing an independent diplomatic apparatus with his own agents. A pro-Slav posture and particular attention to the Balkan area characterized Czartoryski's activities in the next decade.

Appended to Hahn's fine study are several previously unpublished memorials by the prince from the Czartoryskis' Library in Cracow, a comprehensive bibliography, a useful summary of the book in Polish, and an unfortunately garbled one in English.

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JAN TOMASZ GROSS. *Polish Society under German Occupation: The Generalgouvernement, 1939–1944*. Princeton: Princeton University Press. 1979. Pp. xviii, 343. \$20.00.

It is always encouraging to find a pure "social scientist" concentrating his labors upon a subject normally considered to be the monopoly of historians and publicists. The sociologist Jan Tomasz Gross provides an excellent example of the rich harvest awaiting historian and social scientist alike with his masterful analysis of the German occupation of Poland during World War II. He has produced a sorely needed, objective, and reasonably dispassionate account of an extremely emotional and vital topic in contemporary history.

The author's main thesis is that a society cannot be destroyed forcibly by an alien group, unless that society's population is totally annihilated. After perceptively analyzing the theory and practice of Nazi occupation policy, Gross examines the Polish population's response in terms of alternative collective behavior patterns that developed to replace those prohibited by the Germans. Several interesting and important points emerge from this scrutiny. First, far from being the coldly rational, calculated, monolithic approach popularly ascribed to any feature of the Nazi regime, German occupation policy was vague, hazy, and often undefined, depending for much of its essence on momentary arbitrary decisions by its executors. This lack of a clearly delineated program was both cause and effect of sharp internal rivalries and resultant splits within the occupation leadership, which mirrored the situation within the Nazi movement itself. Second, Gross shows how the Germans managed the Polish econ-

omy so incompetently as to turn a potentially advantageous element of the Nazi war machine into a major burden. Third, there existed a very high degree of corruption throughout all levels of Polish life—corruption that the Germans knew about and, surprisingly, tolerated, thus encouraging it to flourish. Here the Germans made a serious mistake, for corruption soon became one of the most important alternative bonds holding Polish society together after normal integrating forces had been obliterated. The final point worth discussing is probably the most important, especially for scholars accustomed to looking at World War II resistance movements as counteroffensive forces directed against the domineering conqueror-occupier. The author argues that the Polish underground was not primarily an anti-German conspiratorial operation, “but rather a substitute for Polish society in all its functional diversity and plurality of organizational forms” (p. 283). With the traditional social structure in a shambles and normal living impossible, the underground apparatus directed its attention and activities mainly toward the Polish population and only secondarily against the Germans: “It was essentially a norm-creating institution for those who joined it and also for those who remained outside of its network” (p. 256).

The many merits of this book notwithstanding, several shortcomings somewhat hamper its total effectiveness. Despite the enormous amount of research material available in both English and Polish, let alone in German, the source base is quite narrow. Although use of recently published primary and secondary materials on prewar and wartime Poland may not have undermined the author’s hypotheses and supporting proofs, their absence does open his findings to serious question. This is especially true of the rich archival and printed sources available in the Polish archives. This in turn may account for some curious omissions in the discussion: the Roman Catholic Church, such a preponderant force in Polish life, is scarcely mentioned; and the large Belorussian populace of eastern Poland is ignored completely in the examination of the relationship between ethnic Poles and the national minorities, despite the fact that the Belorussians played an important part in this facet of the occupation and could have been profiled as readily as the Ukrainians. Finally, the book’s organization is awkward and cumbersome. It seems more logical to place the final two chapters, “The Underground as a Social Movement” and “The Underground as a Polity,” earlier in the work, to be followed by specific discussion of key facets of both dimensions of the occupation. This would seem to provide a more coherent, free-flowing account and in turn would enhance the book’s usefulness.

But these reservations are mild when measured against the pedagogical value of the book. With occasional minor lapses, Gross has succeeded in avoiding the inane jargon that so many social scientists affect, and the result should be counted among the standard studies of any resistance movement for some time to come.

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V. STANLEY VARDYS and ROMUALD J. MISIUNAS, editors.
The Baltic States in Peace and War, 1917–1945. University Park: Pennsylvania State University Press. 1978. Pp. viii, 240. \$12.50.

This volume of essays, which emerged from the papers presented during the 1973–75 period at several conferences on Baltic studies in the United States and in Europe, is saved from the fate of being a “non-book” mainly by two considerations. The first is its introductory chapter, which successfully places the varied contributions to the history of the Baltic states in general, and of Estonia, Latvia, and Lithuania in particular, within a broader chronological and functional context. Secondly, the four-part chronological grouping of the essays within the established 1917–45 framework is also helpful in this regard. Significantly, there is no general concluding chapter.

Three essays—by Olavi Arens, “The Estonian *Maapäev* during 1917”; by Charles L. Sullivan, “The 1919 German Campaign in the Baltic: The Final Phase”; and by Aba Strazhas, “The Land *Oberost* and Its Place in Germany’s *Ostpolitik*”—deal together, as it were, with the aspects of the Baltic road to independence, pointing to the failures of German policy in the East and the concomitant successes of the local Baltic peoples. Two essays—one by V. Stanley Vardys, “The Rise of Authoritarian Rule in the Baltic States”; and the other by Michael Garleff, “Ethnic Minorities in the Estonian and Latvian Parliaments: The Politics of Coalition”—present aspects of Baltic political and constitutional development, pointing to the rather mild characteristics of the regimes established, particularly considering the Hitlerite and Stalinist variants operating at the time in their vicinity.

Four of the essays—by Alexander Dallin, “The Baltic States between Nazi Germany and Soviet Russia”; by David M. Crowe, Jr., “Great Britain and the Baltic States, 1938–1939”; by Julius P. Slavenas, “General Hans von Seeckt and the Baltic Question”; and by Edgar Anderson, “The Baltic Entente: Phantom or Reality?”—analyze aspects of the international position of the Baltic states in Europe between the two world wars, discuss their role

in the calculations of the great powers, and also deal with their own unsuccessful efforts to stay independent. And, finally, four essays—Boris Meissner's "The Baltic Question in World Politics"; Dennis J. Dunn's "The Catholic Church and the Soviet Government in the Baltic States, 1940–1941"; David Kirby's "Morality or Expediency? The Baltic Question in British-Soviet Relations, 1941–1942"; and Romuald J. Misiunas's "Soviet Historiography on World War II and the Baltic States, 1944–1974"—attempt to show the impact of World War II, together with the goals of the Soviet Union in the Baltic area. They deal with the international reactions to the Soviet occupation and incorporation of the Baltic republics in 1940 and with the views and positions of Churchill, Eden, and Roosevelt on this question in 1941–42. The essay by the junior editor gives a useful overview of the kaleidoscopic changes in Soviet presentations of the history of World War II, showing that the old adage, "history is current politics extended backwards," still accurately describes current Soviet practices.

All of the essays are competently written by experienced and well-informed scholars. The contributions are carefully annotated and documented from Western archival and memoir sources. They include only a sampling of Soviet documentation, however, since Soviet ideological and political sensitivity still does not permit free access to Western scholars to their extensive archival holdings.

The collection of essays, capably edited by Vardys and Misiunas, can be fruitfully used to fill in the existing gaps in our knowledge about the Baltic area. It can also serve as a useful guide to further research. The essays themselves, individually and *in toto*, present an effective counterbalance to the standard, heavily ideological, and politically colored Soviet interpretations of recent historical developments concerning the Baltic states.

BENEDICT V. MACIUIKA
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E. A. RYBINA. *Arkheologicheskie ocherki istorii novgorodskoi torgovli X–XIV vv.* [Archeological Essays on the History of Novgorod Trade, Tenth to Fourteenth Centuries]. Moscow: Izdatel'stvo Moskovskogo universiteta. 1978. Pp. 164. 95 k.

The long and detailed excavations of medieval Novgorod have provided historians with some of the most interesting information on life in Rus'. E. A. Rybina, a student of V. L. Iarin, has concentrated on the application of the excavations' finds to questions of Novgorod's import trade, and four previously published essays are reprinted here. While the essays are only vaguely related and of uneven

quality, they provide an intriguing glimpse into a part of Novgorod's history that written sources do not treat.

The heart of the book consists of two essays treating southern and western imports. By plotting the occurrence of import wares like walnuts, slate beads, amber, and ceramics in Novgorod's dated layers, Rybina establishes chronological frequency patterns suggestive of trade patterns. Southern imports, which either originated in or passed through Kiev, predominated in the tenth, eleventh, and twelfth centuries, after which their numbers decline steeply. Western imports, by contrast, demonstrate their strongest frequency in the period after the thirteenth century. Rybina explains this correlation by reference to the Mongol destruction of Kiev and the southern trade route in the thirteenth century. Similar interruptions characterize the western and eastern trade, and Rybina attributes these stoppages to the Teutonic Knights and Polovtsy respectively. While perfectly plausible, these explanations are offered as proved, something which Rybina's evidence cannot do.

Two large essays complete the book. The first, the book's weakest, analyzes the birch bark documents for information on trade. To Rybina's credit, she observes early on that some of the most fantastic explanations proposed by L. V. Cherepnin (*Novgorodskie berestiane gramoty kak istoricheskii istochnik* [1969]) must be dismissed. Nevertheless, Rybina herself is reduced to citing documents that mention a series of fish or the purchase of two horses as trade documents.

The final essay is devoted to the historical topography of Novgorod. Rybina studies excavations of the headquarters of the most influential of Novgorod's trading partners—Gotland and the German towns. As Rybina notes, these excavations unfortunately were not completed due to modern construction, but the locations of the Europeans' trade and residence centers are now firmly established.

Most of the book is solid, but several judgments require further qualification. For example, Rybina indicates that the name by which the Novgorodians called the Gotland islanders was *Variagi*. The evidence is slim, and in any case requires consideration of other citations. Also dubious is the contention that a Kievan blockade was responsible for the temporary decline in southern imports at the turn of the twelfth century. Even if there was such a blockade (Rybina cites only thirteenth-century references), it is doubtful that the blockade could have been policed effectively.

Still, the book is stimulating. Rybina's most important conclusions are summarized at the book's end, and a brief bibliography is appended.

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RUSSELL, ZGUTA. *Russian Minstrels: A History of the Skomorokhi*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xv, 160. \$16.00.

The *skomorokhi*, or minstrels, have long been recognized as an integral part of medieval Russian culture, but, as Russell Zguta rightly notes, they have, with few exceptions, been just as long neglected by historians. Any history of these enigmatic minstrels must of necessity include much conjecture, but Zguta's conclusions on their role in Kievan pagan ritual and as conduits of Kievan *byliny* to Novgorod and Muscovy are good working hypotheses. It is to Zguta's credit that what information there is has been brought together in this compact history.

Zguta traces the history of the *skomorokhi* from Kievan Rus' to their decline in the seventeenth and eighteenth centuries. Zguta believes that the conversion of Kievan Rus' to Christianity forced the transformation of the *skomorokhi* from rural pagan priests to minstrel-entertainers. His basic assumption is that they were indigenous to Russian paganism and not the offshoot of Byzantine mime or the German *Spielmänner*, although the former contributed acrobatics and the latter the distinctive dress of Russian minstrels. As pagan priests, Zguta ties the *skomorokhi* to the occult arts of witchcraft, marriage ceremony, and the pagan festivals of the *Rusal'ia* and *Koliada*. Yet Zguta draws no connection between the *skomorokhi* and the *volkhvy*, who are credited with the pagan reaction of the eleventh century and are described as pagans or as pagans in the guise of Manichaeism or Bogomilism. He repeats Vernadsky's conclusion that the *volkhvy* were an urban and upper-class phenomenon, but a sharp distinction between urban and rural paganism, particularly in the pre-Christian era, is doubtful. This raises another serious problem in that the *skomorokhi* are often described as representative of residual paganism and secular entertainment. The degree to which they are either pagan or Christian in medieval Russia is not made clear, and if they continued the pagan tradition, what does secular culture mean in this context?

According to Zguta, the political fragmentation of Kiev in the twelfth century and the Mongol invasions drove the *skomorokhi* north toward Vladimir-Suzdal and Novgorod. Condemned in Muscovy by Maksim Grek, Metropolitan Daniil, and the *Stoglav* in the sixteenth century, the *skomorokhi* flourished in Novgorod, and the reasons for this Zguta ascribes to Novgorod's traditional liberalism and democracy. But this explains little. What is needed is an analysis of the complex historical role of Russian Orthodoxy in Novgorod and Muscovy.

Zguta traces the impact of Ivan IV's devastation of Novgorod on the *skomorokhi* and their dispersal throughout Muscovy. He makes an interesting

analysis of the unregistered *skomorokhi*, who were seen by Muscovite authorities as lawless, and the registered, who ran the gamut from Ivan's personal entertainers to the *strel'sy* and *bobyli*. The *skomorokhi* were proscribed in December 1648 as part of the spiritual reform, Aleksei's own piety, and perhaps also the urban riots. But Aleksei's motives are not fully explained, for, on the one hand, Zguta describes him as pious and, on the other, as a supporter of secularization or Westernization.

The last two chapters show the minstrels' impact upon the *byliny*, music, dance, and theater (especially puppetry). Zguta's work is a welcome addition to the growing literature of medieval Russia.

LAWRENCE N. LANGER
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DANIEL CLARKE WAUGH. *The Great Turkes Defiance: On the History of the Apocryphal Correspondence of the Ottoman Sultan in Its Muscovite and Russian Variants*. Foreword by DMITRII SERGEEVICH LIKHACHEV. Columbus, Ohio: Slavica Publishers. 1978. Pp. ix, 354. \$18.95.

Through his publications in both Western and Soviet journals and his participation in scholarly conferences, Daniel Clarke Waugh of the University of Washington at Seattle has already acquired an international reputation as a specialist in the study of Muscovite manuscripts and literary monuments. Further testimony to his recognized expertise as a textologist and codicologist is provided by the laudatory foreword to this, his first book, by Academician D. S. Likhachev (pp. 1-4). In this highly technical monograph Waugh analyzes one genre of Muscovite *turcica* (literary works concerned with the Ottoman Turks), apocryphal letters of the sultan. By investigating their origin and manuscript tradition Waugh locates these texts within the context of Muscovite literary culture of the seventeenth century. On the basis of the most exacting manuscript analysis, extracted from Soviet (and non-Soviet) archival repositories by prodigious labor, Waugh proves conclusively that all the Muscovite variants are translations from European prototypes, despite some assertions to the contrary. Moreover, scrutiny of the manuscript tradition suggests that as in Europe the texts reappear in patterns that conform to political events in Muscovite and Ottoman history, most notably wars. In his brief conclusion (pp. 187-98) Waugh identifies some implications of his study of the apocryphal correspondence of the sultan for our understanding of Muscovite culture.

Technically and methodologically this monograph is simply a tour de force—parallel texts in several languages, genealogical stemma, charts, graphs, and tables are ubiquitous. The body of the work (pp. 5-198) is followed by the "Apparatus

Criticus": sample texts of genuine and apocryphal letters (pp. 200–21); manuscript descriptions (pp. 222–77), at which Waugh is a master; notes (pp. 278–318); and a selected bibliography, index of manuscripts cited, and index. Waugh is quite modest in presenting the results of his research, honestly willing to revise his own earlier views on specific questions (p. 295, n. 86), frank in admitting gaps in his own manuscript notes (p. 266) and in the manuscript evidence, and insistent on the tentative nature of his conclusions pending definitive additional work. The impartial reader will find it difficult not to be overwhelmed by Waugh's evidence and thoroughly convinced by his arguments. This is a valuable contribution to our knowledge of seventeenth-century Muscovite culture.

Probably only specialists will want or need to follow Waugh's investigations step by step. The general reader can, however, fruitfully turn to the conclusions of each part and the overall conclusion for fully accessible discussions of the results of the more technical analysis.

Waugh eschews more extended interpretation of the significance of the apocryphal correspondence with the sultan until all genres of *turcica* can also be taken into account (pp. 11–12), for each of which the same painstaking and meticulous spade work will be required. One is confident that Waugh will fulfill this need with the same skill and care he has lavished on the apocryphal correspondence. In the meantime Waugh whets our appetites for his future research by raising a host of wider questions about Muscovite culture: the degree to which it was integrated into European culture in the seventeenth century; the processes by which literature entered Muscovy from the West and was then translated and circulated; the differing roles of *turcica* in European and Muscovite culture; the relationship of this translated literature to the development of original "documentary *belles-lettres*" in Muscovy; and the connections between the importation and dissemination of *turcica* and the cultural transformation of Muscovy in the seventeenth century that culminated in the reign of Peter the Great.

As an extra bonus to the reader, the volume contains forty pages of illustrations of European and Muscovite texts (pp. 98–137, list pp. 335–36).

CHARLES J. HALPERIN
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IA. E. VODARSKII. *Naselenie Rossii v kontse XVII-nachale XVIII veka* (*Chislennost', soslovno-klassovyi sostav, razmeshchenie*) [The Population of Russia in the Late Seventeenth and Early Eighteenth Centuries (Quantity, Class Composition, Distribution)]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 262. 2 r. 10 k.

This important book provides data about and tentative answers to fundamental questions about the size, structure, and movement of the Russian population. It briefly analyzes the historiography and character of the problems encountered in working with census and other demographic sources in the Russian context and describes the variety of sources themselves. Ia. E. Vodarskii has filled this text with tables that summarize his findings, not always with the greatest clarity or success. There are maps that define regions and *uezdy* of Russia, a particularly difficult and important accomplishment, and extensive appendixes of data on population in 1678–1719. The administrative map showing boundaries of *uezdy*, *stany*, and *volosti* is a major contribution to our knowledge of Russia in the period. Vodarskii notes that it is the fruit of years of painstaking work. Certainly, it is on the empirical level that his work makes its most important mark, rather than in any new theoretical framework or insight.

This work presents no great surprises in its periodization or its classification of Russia's population. Instead, this is a compilation of Vodarskii's earlier work and a correlation with the analyses of other historical demographers, geographers, and historians. The discussion of the urban population of Russia illustrates the strengths and a few of the weaknesses of this book.

Vodarskii includes an important discussion of the definition of a Russian town, following the "classical Marxist criteria." He argues that Russia's towns fit this model as well as the European towns that initially provided the material for the model. His criteria for a town become rather narrow and exclusive, and, although he puts Russia into a European context, it is at some cost to the understanding of Russian urban development in this period. Towns without a trade and artisan population are found not really worthy of inclusion as part of the total urban population of Russia. The author does not completely ignore other groups of the urban population, but tends to emphasize the townsmen (*posadskie liudi*). Thus, he notes that information on other social groups in the towns is scanty and incomplete but does little to estimate important non-townsmen populations. The result of this is a systematic underestimation of Russia's urban population in the late seventeenth and early eighteenth centuries. His summary table (p. 134) presents an urban male population figure of 2 percent. In fact, the data he presents indicate the level at 4 percent even without a closer estimate of all groups in Russia's towns. The figure may be as high as 6, not 2, percent if all residents are included rather than relying primarily on the juridically urban population.

The discussion of migration is another area of important new data in this work. Vodarskii's figures

clearly show movement away from the non-Black Earth center, the northwest, and north and identify individual areas of migration within various regions. He emphasizes a more "natural" movement, without state impetus, and notes the establishment of new settlements in the south and southeast considerably before the state recognized and fortified these locations. The state's role is greatly de-emphasized in this standard Soviet interpretation.

This book suffers from the complexity of its structure and a lack of clarity in the presentation of its massive and important data. But this is a major source for all historians whose work touches any aspects of population, migration, or urban development.

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A. P. PRONSHTEIN, editor. *Don i stepnoe Predkavkaz'e, XVIII-pervaia polovina XIX v.: Zaselienie i khoziastvo* [The Don and the Steppe of Ciscaucasia, Eighteenth and First Half of the Nineteenth Centuries: Settlement and Economy]. Rostov n/D: Izdatel'stvo Rostovskogo universiteta. 1977. Pp. 239. 2 r. 44 k.

This volume, the first of a projected two-part study, is a collection of short essays by historians from Rostov State University under the direction of A. P. Pronshstein. The geography of the Don basin and North Caucasian steppe (Ciscaucasia) receives special attention, as does the specific effect of colonization on the area's socioeconomic development. The essays are organized under four headings: colonization, agriculture, manufacturing, and trade. Since the same scholars usually deal with a specific region in each of the subdivisions, the final result has more continuity than the ordinary collective undertaking (*sbornik*) authored by Soviet historians.

Colonization was the final stage in the process of gradual annexation that the imperial regime had pursued in expanding to the Black Sea and Sea of Azov in the eighteenth century. With the growth of fortified strong points along the southeast frontier by the century's end, raids by the Turks and Nogai Tatars decreased, enabling the government to pursue a consistent policy of settling the area. One of the main contentions found in this study is that resettlement was a means of alleviating social pressure in Central Russia and the Ukraine, where a growing population pressed on available land and food. In the first decades of the nineteenth century, serfs belonging to the state were encouraged to migrate. Nobles who possessed holdings in the Don and Ciscaucasia were also permitted to transfer their proprietary serfs from other parts of Russia.

It is questionable whether these efforts produced

an adequate flow of settlers. Joining the "legal" arrivals were fugitive serfs, largely from those provinces close to the frontier. Although official policy was to prevent these refugees from registering as members of the Cossacks and thereby gaining permanent sanctuary, their numbers had grown so by the 1820s that local officials paid little attention to the letter of the law and allowed many to stay. Efforts were also made to encourage non-Russian colonists to migrate to the Caucasus through promises of good land and tax benefits.

The eventual impact of this population movement can be seen in the essays describing economic development. Statistical evidence gathered by the authors demonstrates the emergence of a settled pattern of agriculture and trade. Here again the government played a key role, pacifying the frontier so that permanent cultivation could develop and later fostering the growth of trade with the Russian heartland. Although the contributors emphasize the low technical level of agriculture in the region, the fertility of the soil and mild climate enabled production to climb steadily. So-called manufacturing (largely cottage industries) exhibited a more modest increase, apparently causing the authors to devote much of their attention to the mid-century, when the beginnings of capitalist "cooperation" can be detected.

This work does make a contribution as an introduction to the history of a region accorded little space in general accounts. Its utility as a reference work, however, would have been enhanced had more detailed maps been provided. The only maps, found in the end paper, are of little help in locating the specific regions often referred to in the text or the regions populated by various ethnic groups. The aggregate result of colonization and economic growth would also be clearer were the statistics combined in general tables, either at the end of the chapters or the volume itself.

ROBERT D. GIVENS
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V. V. POZNANSKII. *Ocherk formirovaniia russkoi natsional'noi kul'tury: Pervaia polovina XIX veka* [An Essay on the Formation of Russian National Culture: The First Half of the Nineteenth Century]. Moscow: Izdatel'stvo "Mysl'" 1975. Pp. 221. 1 r. 13 k.

This book is divided into four overlapping sections. In the first, V. V. Poznanskii summarizes "Russian culture" at the turn of the nineteenth century and places it in the context of the Russian class and economic structure. The second chapter focuses on the Napoleonic Wars in general and the War of 1812 and its consequences for Russian patriotism in particular. He sketches the liberal ideas held by the pa-

triotic intelligentsia, the self-interested nature of aristocratic nationalism, and the "freedom-loving" national spirit expressed by the "people." In chapter three, Poznanskii demonstrates the origins and implications of Official Nationality; and the final part, which concentrates on the late 1830s, is held together by constant reference to the Slavophile-Westernizer debate. The Decembrist movement is the book's most consistent focal point.

Intended as a "necessarily brief and schematic" overview of Russian culture during the first half of the century, this book reads like a long and only slightly analytical essay. Non-Marxist scholars are likely to be put off, for Poznanskii insists as a preliminary qualification that the study of the formation of a national culture in Russia "is possible only on the basis of a Marxist understanding of the structure of the social conscience" (p. 8). That warning may explain why this reader is still puzzling over the purpose of the book!

Filled with an inordinate number of quotations from Lenin and the nineteenth-century socialists Herzen, Belinskii, and Chernyshevskii—and with many other seemingly random extracts from individuals who contributed in various ways to the creation and interpretation of Russian culture—the work has no central tale. Its eclectic nature can be illustrated by the fact that Poznanskii pays little attention to the forums in which his personae expressed their ideas; that is, he cites from such journals as *Biblioteka dlia Chteniia* (1834–65), *Severnata pchela* (1825–64), *Vestnik evropy* (1802–30), and *Syn Otechestva* (1812–30) but ignores the overall role of these and other journals as vehicles of national feeling. All individual contributors to the evolution of a Russian national consciousness are treated favorably, but those who added the liberty of mankind to their Russian nationalism are the greatest heroes of all. The fact that Russian nationalism often contradicted the principle of freedom and independence—even among the Decembrists in regard to Poland—is only barely mentioned.

Poznanskii pays little heed to important movements that were international in character: pietist movements, the Russian Bible Society, Freemasonry, and so on. The fact that for the first twenty years of the century Russian culture was intricately involved in continent-wide trends—and that even under Nicholas I, the influence of German philosophy on the minds of young Russians was overwhelming—are realities that he sets aside. Granted, the author is studying the growth of an indigenous culture, but he cannot do it successfully in a vacuum.

There are bits and pieces of interesting reading here, but once on to something worthwhile, Poznanskii tends to shift to another subject. This is es-

pecially the case when he touches upon education—its organization, purpose, and content. The attempts of a variety of ministers of public instruction to shape the thinking of young Russians through state-directed programs are dealt with in far too cursory a manner. His remarks about the arts, theater, and music (with the exception of material on M. I. Glinka) seem to be thrown in almost incidentally. To add to the reader's frustration, the absence of an index makes it very difficult to trace an idea—or personal contribution—throughout the book.

The subject matter chosen by Poznanskii warrants thorough study; unfortunately, this book falls considerably short of the mark.

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Carleton University

M. N. PEUNOVA. *Etika N. V. Shelgunova* [The Ethics of N. V. Shelgunov]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 215. 95 k.

N. V. Shelgunov was among the several dozen most widely read Russian radical journalists of the 1860s. An admirer of Belinskii and Chernyshevskii, Shelgunov wrote his most interesting works between 1860 and 1868, suffered imprisonment and exile for his views, and continued his journalistic career sporadically, when so permitted, until his death in 1891. The term "ethics" as used in this title refers strictly to the humanitarian premises that dominated his thinking.

Unfortunately, Shelgunov was not an original thinker, and what strikes a reader familiar with this field is the amazing similarity of his views to those of other Russian radicals of his generation. Like them, Shelgunov agonized over the individual-social dilemma of utilitarianism and muddled around in the dichotomy between socioeconomic determinism and Lavrov's critically thinking individual, between the richly developed personality and the plodding masses. Shelgunov's views on pedagogy are interesting but scarcely unique, and even in his attacks on Tolstoy (as *Narodnik*, individualist, and moralist) he mirrors contemporary radical opinion. Perhaps his greatest achievement is an interesting analysis of public opinion and its effect on individual behavior and thinking.

M. N. Peunova's major efforts are devoted to pointing up similarities between Shelgunov's thinking and that of the Marxists. In this context her subject presents fertile soil, since he adopted his generation's tendency toward materialism and environmental determinism, sticking to his guns even when others abandoned theirs. Peunova's foregone conclusion is that his ideas "prepared the soil" for

Marxism (p. 4) but that he remained hidebound by his times and by his latent commitment to what she terms "idealism."

The book is filled with irritating assessments of Shelgunov from the Marxist viewpoint and continually labels attitudes as "correct" or "incorrect" in accordance with their approach to "scientific" analysis. The chronological development of Shelgunov's thought is not clear, and thus the author's important assertion that he became more, not less, "materialistic" in later years remains unsubstantiated. Contemporary Western thinkers are seldom given the credit they deserve for their influence on the Russian scene. In her efforts to read Marxism into her subject, Peunova is quick to deduce socialism from Shelgunov's statements, but from the proofs adduced here, one cannot really tell if she is right. There are not enough footnotes, there is no index, and a bibliography of Shelgunov's works is not attached.

Peunova's most interesting contribution lies in her analysis of the conflicting ethical premises of utilitarianism (here called "rational egotism") and socioeconomic determinism: that is, of an ethical system centered around man as unique biological being and one coldly conditioned by the immutable laws of historical development. We are back to individual versus society, freedom versus determinism, personality versus sociology. Greater men than Shelgunov have tried to reconcile these concepts and failed.

This is a useful book for anyone seeking the views of a typical "revolutionary democrat," but its conclusions must be accepted with caution.

DEBORAH HARDY
University of Wyoming

EDWARD ACTON. *Alexander Herzen and the Role of the Intellectual Revolutionary*. New York: Cambridge University Press. 1979. Pp. x, 194. \$18.95.

A. I. VOLODIN et al. *Chernyshevskii ili Nechaev? O podlinnoi i mnimoi revoliutsionnosti v osvoboditel'nom dvizhenii Rossii 50-60-x godov XIX veka* [Chernyshevskii or Nechaev? On the Genuine and the Sham Revolutionary Character in the Liberation Movement of Russia of the 1860s and 1870s]. Moscow: Izdatel'stvo "Mysl'." 1976. Pp. 294. 1 r. 18 k.

In our age of revolution, the flow of historical studies of radical intellectuals seems assured. We join the past and the present in a search for answers to the ideological, tactical, and moral questions associated with revolutionary change. The two books under review, concentrating on related aspects of the Russian revolutionary movement, speak to these

questions but from contrasting assumptions and conceptions about the study of the past.

Edward Acton offers an intellectual biography of Alexander Herzen, a man whose life and thought were inextricably intertwined. Tracing the impact of private tragedy and disappointing public events on Herzen's thought and activity from 1847 to 1863, Acton explains how one outstanding individual worked and reworked his own ideas and aspirations and adjusted his actions accordingly. Herzen's effect on society, or on the larger historical process, is not denied, but such is really not Acton's concern. It is the biography that matters most.

Within the limits of this approach, Acton performs admirably. He is clearly at home in the materials on Herzen's career, and he writes in a concise, attractive style. The picture he draws of a life marred by personal loss and frustration is sensitive and sympathetic. In turn, he builds a strong case for his version of the development of Herzen's thought. Taking issue with Martin Malia's views, Acton denies Herzen's lifelong messianic and revolutionary nationalism and stresses the impact of family tragedy on his thought. Indeed, the compounded family misfortunes of 1852 are seen as central in undercutting the personal optimism that sustained Herzen's belief in a predestined socialist regeneration and the guaranteed triumph of the rational over the irrational in the historical process. Once Herzen had reached a more realistic and pragmatic position, by the mid 1850s, he was free to question whether a peasant rebellion, however morally justified, would in fact be creative. From this position, he found a new role for himself after the Crimean War, encouraging reform from above and acting as a brake on younger radicals who called all too quickly for a peasant rebellion. Only belatedly and reluctantly, after the collapse of his reform hopes, did he join the radicals, but by then it was too late to be an effective leader.

One might object to the narrow focus of Acton's study, which leads him to underplay some important questions. Herzen's intellectual relationship to Nicholas Ogarev, for example, or his actual participation in attempts to form an underground organization, might both have been given further treatment. Yet that same narrow focus also adds to the persuasiveness of his version of Herzen's intellectual pilgrimage.

Chernyshevskii ili Nechaev?, by A. I. Volodin, Iu. F. Kariakin, and E. G. Plimak, offers a totally different kind of concern with the past. The problem posed by the book is to distinguish between "genuine" and "sham" forms of revolutionary thought and action in the Russian movement of the 1850s and 1860s. The study is historical insofar as it examines the careers of several revolutionaries of the pe-

riod, but the book's main thrust is present minded and ideological. The authors seek to define proper revolutionary action, as opposed to its terrorist or anarchist perversions, and to establish beyond question the connection of their own Marxist-Leninist tradition with the brighter rather than darker side of the revolutionary movement. Thus, the past is approached for lessons and for validation of a present concern.

Not unexpectedly, N. G. Chernyshevskii, to whom almost three-quarters of the book is devoted, ranks highest in the utopian, pre-Marxist tradition. His writings reflect humane concern and the highest morality and, whereas he accepted the need for forceful revolutionary action, he did so with appropriate restraint, a realistic sense of timing, and a sophisticated awareness of probable outcomes. In contrast, the authors' briefer treatment of D. V. Karakozov and S. G. Nechaev serve as major examples of what should *not* be done. Counterproductive acts of terror, impatience leading to inappropriate timing, and deceitful immorality in the name of revolution, all come under the authors' severe criticism. For those nineteenth-century conservatives who lumped the genuine and sham revolutionaries together or for "bourgeois" historians who suggest some reflection of the sham tradition in Leninism, their criticism is no less severe.

Western specialists will have most interest in the chapters defining Chernyshevskii's position. In one of these a close textual analysis is given of his translation of F. K. Schlosser's histories of the eighteenth and early nineteenth centuries for clues on Chernyshevskii's own views of revolution. Another chapter suggests a second level of meaning to his novel *What Is To Be Done?* Both arguments test one's credulity but deserve serious consideration. So too, attention should be given to the authors' description of revolutionary action in the early 1860s. Building on the best of Soviet scholarship, they present a realistic picture that suggests a greater similarity between Soviet and non-Soviet interpretations than appeared a decade ago.

WILLIAM F. WOEHRLIN
Carleton College

SEPPO ZETTERBERG. *Die Liga der Fremdvölker Russlands, 1916-1918: Ein Beitrag zu Deutschlands antinussischem Propagandakrieg unter den Fremdvölkern Russlands im Ersten Weltkrieg.* (Studia Historica, number 8.) Helsinki: Suomen Historiallinen Seura. 1978. Pp. 279.

By now it has become well known among historians of the Russian Revolution that during World War I the German government, in addition to its military efforts on the front, also mounted a sizable uncon-

ventional war—let us call it a propaganda campaign against the Russian imperial government. The sensational findings of the late 1950s about German money flowing into the Bolshevik treasury now are considered established facts by most historians. But the Bolsheviks were not the only recipients of German largess. The representatives and parties of Russia's nationalities, disgruntled with the Romanov Empire, were equally an object of the German Foreign Office's attentions. One German approach to the nationalities receives a thorough documentation in the work under review.

That the German government would attempt to befriend Russia's nationalities, according to Seppo Zetterberg, was not surprising at all, for the nationality fissures of the empire had been studied by most important foreign offices of Europe long before the war. When the war broke out, the German strategists did not need to invent a new policy but only to develop tactics to put the existing one into effect. Although the nationality fissures potentially were more corrosive to the structure of the empire than the Bolsheviks, the German efforts to exploit them did not have the same success and significance.

At the outbreak of the war numerous exiles from the Caucasus, the Ukraine, Poland, Belorussia, and the Baltic Provinces, who were residing in Germany, neutral Sweden, Switzerland, and Denmark, desired to have their countries liberated from the stranglehold of the Russian bear. The goals of the two groups—the German strategists and the exiles—were identical, and therefore the ensuing cooperation was natural. The outcome of this cooperation was the League of Russia's Nationalities. Although much of the exiled nationalists' enthusiasm went into the efforts of the league, the author emphasizes that the main organizational, not to mention financial, support came from the Germans. The Germans' problem was how to get the league to pursue pro-German policies without acquiring the image of a German propaganda agency.

The high point of the league was the Nationality Conference of June 1916 in Lausanne. The chief weakness of the league was that its nationality representatives, though very able leaders, only represented themselves and had no parties or large following in their home countries. The February Revolution in Russia changed the status of the league: it was its death knell. In a way the league had outreached itself—its demand for national independence and the dismemberment of the Russian Empire corresponded more to Germany's war aims than to the programs promulgated by the nationalists and their parties within the empire itself. After the February Revolution, Russia's nationalities demanded autonomy in a federative state rather than

separation from Russia, which the league had demanded.

The author has done very thorough and authoritative work in unraveling the skeins of the league's existence. His book fills in an important gap in our understanding of Russia's revolution and Germany's policies during World War I.

ANDREW EZERGAILIS
Ithaca College

T. F. KUZ'MINA. *Revoliutsionnoe dvizhenie soldatskikh mass Tsentra Rossii nakanune Oktiabria: Po materialam Moskovskogo voennogo okruga* [The Revolutionary Movement of the Soldier Masses of the Center of Russia on the Eve of October: Based on Materials of the Moscow Military District]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 309. 2 r. 70 k.

Although Soviet scholarship has always devoted a good bit of attention to Bolshevik penetration of the army in 1917, the quality of these studies has not, in general, been very high (exceptions are the works of V. I. Miller, M. S. Frenkin, and E. N. Burdzhakov). Although they invariably use archival materials inaccessible to foreign scholars, only tantalizing snippets serve to illustrate the well-established canons of Soviet historiography. (Even published sources, particularly of the rich decade of the 1920s, are underused.) Second, although the entire front from Finland to the Caucasus has been covered, the garrisons of the interior have remained virtually untouched, a serious omission, since Bolshevik influence originated in the turbulent, politicized garrisons and reached front units via replacements, returning wounded, and NCO and officer trainees. With the recent work of A. M. Andreev, the present study goes a long way toward redressing the imbalance; but even more rewarding is the new standard in the exploitation of archival materials. T. F. Kuz'mina's study makes thorough use of the archives of the Moscow Military District, which embraced all of the central provinces from Tver to Kharkov, as well as of the mobilization section of the general staff. The Moscow District is crucial in that it consisted exclusively of the agricultural provinces of central Russia, allowing one to observe the social process in its most undiluted form. The mobilization data for the first time afford a concrete picture of the mode of sending replacements to the front: the author offers not only a complete list of reserve regiments, locations, size of garrisons, and number of replacements by month and origin, but even the front units that they supplied, making possible a correlation of radicalized units front and rear.

Although one could write at length on the virtues

of this study, its major contributions must suffice. First, it makes clear that the July crisis was part of a nationwide process of radicalization directly connected to the accelerated sending of replacement units to the front in connection with the June offensive (Alexander Rabinowitch has confirmed this for Petrograd, Kuz'mina for the entire central region). The turbulent elements most susceptible to instant bolshevization were the recovering wounded (roughly 30 percent of many units) and soldiers over forty, who had been released for field work and recalled. Second, unlike Petrograd, the major shift of garrison troops to the Bolsheviks took place *after* the July crisis, because the Bolsheviks were able to capitalize on the attempts of authorities, with the cooperation of local soviets, to restore military discipline, officer authority, and regular training duties. Rabinowitch has already modified the conventional picture that Bolshevik fortunes were at a low ebb until the Kornilov days, but for the garrisons of the central region, the scheme does not apply at all. Until July, most soldier soviets were under the secure control of SRs, but, as a direct result of unrest and punitive expeditions against recalcitrant reserve regiments who did not wish to be shipped to the front, the soldiers went over en masse to the Bolsheviks.

The study also offers a comprehensive overview of the impact of the Kornilov affair, demonstrating that the galvanizing of soviet-oriented elements into various revolutionary defense committees, in which local Bolsheviks invariably played a key role, was a universal process, not simply of the major urban centers (Kuz'mina identifies eighty-one such committees, twice the number previously established by Soviet investigations). More than any previous study, this one confirms the secure hold the Bolsheviks attained by October on the key soviet constituencies, which were easily translated into local transfers of leadership and a near majority at the Second Congress of Soviets, giving firm underpinning to the proclamation of soviet power. The study fails on two important questions, however: it ignores entirely the process of "self-demobilization" of the garrisons (which was surely reflected in the reports Kuz'mina otherwise extensively cites); and second, though the slogan "soviet power" is given its due, the Second Congress of Soviets is not mentioned once, though surely the question of representation at the congress was a major issue exploited by local Bolsheviks to force by-elections and overturn Menshevik-SR majorities in local soviet executive committees. In fact, the study stops short of dealing with the ramifications of the October Revolution, particularly of local transfers of power in which the garrisons certainly played a role. Other themes are neglected or overschematized to suit official his-

toriography, but on the whole this is one of the better Soviet monographs on 1917.

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RICHARD K. DEBO. *Revolution and Survival: The Foreign Policy of Soviet Russia, 1917-18*. Toronto: University of Toronto Press. 1979. Pp. xiii, 462. \$25.00.

This well-appointed volume, including the luxury of footnotes at the bottom of the page, ventures upon historical ground plowed over by numerous predecessors. It is a conscientious study of Soviet external affairs in the year following the Bolshevik Revolution (the subtitle is somewhat presumptuous since the emphasis, if judged solely by a word count, is upon diplomacy). E. H. Carr, George F. Kennan, John W. Wheeler-Bennett, and a host of less prominent historians pioneered the way, and Richard K. Debo has graciously conceded as much in the introduction. With such a profusion of talent having pre-empted the field, it is legitimate to inquire whether there is room for yet another volume, especially one that recapitulates the familiar story of the Brest-Litovsk negotiations and the genesis of Allied intervention. Anticipating the question, the author provides a forthright answer by maintaining that his precursors wrote at a time when relatively few primary sources were available and that, among other material, he has utilized the archival depositories of Washington, London, Paris, and Bonn. This new documentation has enabled him "to focus on the development of Soviet foreign policy itself rather than viewing it simply as a foil against which other governments pursued their interests in Russia" (p. xii). His objective is laudable, if not always attainable. At any rate, his diligence has succeeded in illuminating formerly obscure nuances of Soviet policy; and when his own research becomes redundant he does not hesitate to defer to the appropriate secondary authorities. But his implication that the time is ripe for a fresh appraisal of Soviet foreign policy is flawed: the Kremlin archives remain closed to independent scholarship, and the publication of Soviet documentary collections has furnished additional details without gratifying our desire for "inside" information emanating from the highest level, that is, Lenin and the top echelon of his party colleagues.

Lenin emerges, unsurprisingly, as the *deus ex machina* of Bolshevik foreign policy. There is a tendency to inflate his skill—not that the interpretation is uncritical—and he emerges as a consummate diplomatist, fully matching his stature as a political tactician. Even under inept leadership, however, Russia could hardly have failed to benefit

from the military confrontation of the Allies and the Central Powers. One could have wished for a more human portrayal of the leading protagonist—indeed the dramatis personae seldom appear as more than fleshless symbols of government policy—but perhaps this is the province of biographers and popular writers. The work makes few concessions to the general reader despite clear and vigorous prose occasionally spiced with a pungent phrase or an apt metaphor. The narrative, while demanding no arcane knowledge, is closely packed and presupposes some familiarity with the revolution and civil war. A map or two would have helped. But professional historians, certainly those who write monographs published by university presses, need not lust after the broad reading public. *Survival and Revolution* is a provocative and in most respects highly satisfactory achievement. Specialists will consult it with profit and perhaps with moments of pleasure.

ROBERT D. WARTH
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V. P. DANILOV. *Sovetskaia dokolkhoznaia derevnia: Naselenie, zemlepol'zovanie, khoziaistvo* [The Pre-Collective Soviet Village: Population, Land Tenure, Household]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 317. 1 r. 80 k.

N. K. FIGUROVSKAIA. *Agrarnye problemy v sovetskoi ekonomicheskoi literature 20-kh godov* [Agrarian Problems in the Soviet Economic Literature of the 1920s]. (Problemy Sovetskoi Ekonomiki.) Moscow: Izdatel'stvo "Nauka." 1978. Pp. 257. 1 r. 40 k.

V. A. SIDOROV. *Klassovaia bor'ba v dokolkhoznoi derevne, 1921-1929 gg.* [Class Struggle in the Pre-Collective Village, 1921-29]. Moscow: Izdatel'stvo "Mysl'." 1978. Pp. 245. 1 r. 10 k.

V. P. Danilov is the dean of Soviet students of peasant life during the New Economic Policy of the 1920s. His latest book, which summarizes a number of his articles over the last few years, is a clear and elegant summary of the main lines of peasant social and economic development in the USSR on the eve of collectivization. He employs a minimum of obscuring catchwords, such as "kulak," and concentrates on how the village functioned as an economic and social unit. The book is divided into the three sections of the subtitle—population, land tenure, and households. Aware of the enormous regional differences, he attempts a reasonable, and persuasive, description of the whole. Necessarily, the small peasant household family unit, organized into village communes, dominates the picture. One of his important points, which he has made previously but which still comes as a shock to preconceptions,

is that peasant agriculture in the 1920s was on the whole remarkably successful in its own terms. This follows from his analysis of the effects of the agrarian revolution and the economic revival of the middle 1920s. His careful discussion of the number of households indicates that the negative effects of splintering were less than is usually supposed; household units increased from sixteen million to twenty-four million, rather than twenty-five. Productivity improved considerably compared to the prewar period and reached its high point in 1925 and 1926. In making the comparison Danilov quite properly includes prewar sharecropping among peasant farming rather than that of landlords. Separate peasant households in the 1920s increased their acreage, while the existing collective farms were primarily planted on familiar and long-used ground.

Danilov demonstrates the familiar weaknesses of small household farming in communes—the multitude of strips, their distances from the homestead, the obstacles to the introduction of new methods of cultivation—but his point is that the agrarian revolution did indeed lead to a rational liberation of peasant activity. The backwardness and misery of peasant life is brought out by the fact that everyone over the age of ten had a place in the rural work force. Danilov discusses social structure by looking at the distribution of occupations within the “family cooperative.” In the nature of things, over half of the *batraki*, full-time wage earners, the authentic agrarian proletariat, were under twenty—that is, they were people who had not yet started a family and who were therefore something less than full members of the commune. Danilov reminds us of the rigorous connection between family size and social position: poor peasants had small families, rich peasants had large.

The nationalization of land resulted in its effective disposition by the village commune, or *mir*, which was now called the “land society,” governed by a rural assembly, and granted a renewed lease on life by the revolution. As an authentic peasant organization it was supreme, and one of the more important and generally unsuccessful campaigns of the government was to bring the *mir* under the control of the rural soviets, precisely to break up the impenetrable unity of the *mir*. The authorities felt obliged to intervene if the *mir* repartitioned the land among its members too often, thus interfering with production, or not often enough, thus avoiding the egalitarian impulse of the revolution. They also tried to regulate repartitions along familiar class lines, so that poor peasants were to receive the best and nearest land and kulaks the worst and farthest—criteria that the *mir* found inane or impossible. Even when the criteria were formally ob-

served, they were frustrated by the molecular processes of rural life; poor peasants, without animals and tools, had to cede their better land to richer peasants under a wide variety of disguises. Initial NEP legislation was inclined to codify what the peasants themselves wanted. Along with the recovery of agriculture came attempts to introduce normative rules that made better economic sense. The supervisory rights of the rural soviets over the peasants’ *mir* became especially contentious in 1927–29 as the governing bodies themselves searched for new principles.

Danilov examines at some length the two visible alternatives to small-scale household farming within the *mir* that were present in the middle and later 1920s. One was the formation of separate farms, *khutora* and *otruby*, the model offered by the prewar Stolypin program. The other was the appearance of various forms of collective farming, state farms, co-operatives, and so on. On the face of it, neither was especially favored by the peasants, who were busy absorbing the results of the agrarian revolution. At the beginning of 1927 only about 1 percent of the land was worked cooperatively, and that under unfavorable circumstances. The movement toward entrepreneurial forms of agriculture was little more successful, given the combination of peasant community resistance and implacable government hostility. Danilov’s conclusion is that while peasant agriculture made considerable progress over prewar norms, it was unable to increase production sufficiently to meet the demands of the country. The crisis that emerged at the end of the 1920s was not generated by internal contradictions within the agrarian sphere by itself. Consequently, one may presume, collectivization was the result of pressure by the state. Danilov’s book is a first-rate description of how the peasants managed to cope before being pushed aside by exogenous requirements.

N. K. Figurovskaiia’s essay is concerned with urban perceptions about the peasants during the 1920s. Its interest lies in the tangential recovery for Soviet readers of some sense of the alternatives that seemed to exist during NEP. She is poignant about the silence that fell for over two decades after 1930. Her discussion, however, of the issues during NEP is locked into a distorted pattern that makes it impossible even to describe the views of the most important contemporary students of village life. She is not really concerned with the economic problems but rather with their political dimensions. Susan Gross Solomon’s recent study, *The Soviet Agrarian Debate*, is much more forthcoming on some aspects of the material, though it does not deal with as wide a range of topics as Figurovskaiia.

V. A. Sidorov’s book is an old-fashioned, unreconstructed treatment of Soviet agrarian affairs, re-

plete with castigations of kulaks, unspecified apart from their moral failings and their recalcitrance to the policies of the government. Yet, for all of his heavy breathing about class struggle within the village, he too feels that collectivization was less a process emanating from agriculture itself than a requirement coming from external demands upon the village that could not be met by the peasants using their old forms of organization—that is, a requirement coming from the needs of urban industrialization. The rationale for it, therefore, lies in the area of intersectional relations of the economy more than in intravillage social struggle. If so, it seems particularly unpleasant to label those who failed to rise to the challenge as vile enemies of the regime and their fellows, when the real difficulty was that they could not understand its purposes, let alone apply themselves to them. Sidorov's archival references remind us of the huge amount of factual material that remains to be brought into circulation to form the basis of judgment about what happened at the end of the 1920s.

Two comments seem in order. One is terminological, with important methodological implications. The neologism *dokolkhoznyi*, “pre-collective,” to describe Russian individual peasant household farming appears to have become accepted usage by Soviet writers. By employing it, the only problem is to explain how peasants made the transition from a lower, incomplete stage to a higher more perfect form: the result is inherent in the adjective. Half a century after collectivization that might be thought legitimate, but it distorts the historical problems of the 1920s to the point of absurdity. The other comment is that the conceptual scheme of a “manufacturing stage” in collective farming, first proposed by Stalin, has been anonymously revived. Since the reference is an analogy to the discussion by Marx of “manufactures,” with his litany of horrors about the extension of “absolute surplus value,” one wonders whether Soviet writers are being ingenuous or Aesopian. My guess is that it depends upon the intelligence of the writer.

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AFRICA

L. H. GANN and PETER DUIGNAN, editors. *African Proconsuls: European Governors in Africa*. New York: Free Press. 1978. Pp. xi, 548. \$29.95.

This book is the latest volume in an ongoing effort by Peter Duignan and L. H. Gann to produce a total history of European colonialism in Africa (there

have already been one synthetic work, a five-volume set of topical essays, and now this and at least three other studies of colonial elites). It is also a collection of twenty-two biographical articles by seventeen authors varying greatly in their approach.

The general contribution that the editors claim they are making with this volume is to “raise some of the most significant questions which can be posed regarding modern Africa,” since they think the governors discussed here “have as good a claim to be regarded as the state builders of modern Africa as the African nationalists who later took over from the colonial regimes” (p. 16). This is hardly a very original or penetrating comment and appears relevant only because the “African interpretation” with which the volume concludes (a device used in several of Duignan and Gann's previous collections) makes the equally unimpressive argument that the governors were nothing more than a group of authoritarian misfits working out their personal psychological problems in an Africa that had little need of them.

The substantive center of the book is far better than either the editors' introduction or the critic's conclusion suggest, but the book as a whole does suffer from failure to place the colonial experience within the kind of political and economic framework that might suggest its major historical relevance. Gann and Duignan, along with the authors chosen to write the introductory essay on each national group of governors, move somewhat in this direction by analyzing the social background of the men sent out to administer Africa (essentially a provincial bourgeois “service class” rather than either aristocratic or mercantile-industrial bourgeois, as is often claimed). This observation allows the editors to repeat their arguments against “Marxists” and tell us again how convinced Marx was of the modernizing effects of colonialism. They do not, however, come to grips with notions of social imperialism and dependent underdevelopment that are the mainstay of contemporary radical critiques of colonialism. Moreover, even the individual articles pay relatively little attention to economic issues beyond the building of infrastructures and the protection of African peasants against white settlers. It is particularly unfortunate that the manifestly close ties that existed between administrators and private-sector colonial interests in the Belgian and French empires are not explored (though touched on by Bruce Fetter) in the sections devoted to governors from these two African spheres. The only article with a clear economic analysis is one by Woodruff Smith on Julius Graf Zech, governor of the minor German colony of Togo.

In general, the individual articles in the book are carefully researched and well written; twenty to

thirty pages seems the ideal length for the treatment of colonial biographies. One drawback to the authors' expertise is that they have chosen to write about figures who warrant intensive individual study. We are thus presented with an array of heroic empire-builders, exemplary developers, and even one or two decolonizers, many of whom were distinguished for actions other than those carried out while functioning as governors and few of whom are really representative of their genre. Perhaps no governor could be fully representative, but it might have helped to throw in a few mediocrities or even downright failures and crooks.

Despite the atypically lofty stature of the figures chosen, most of the authors appear to maintain some critical distance from their subjects: John E. Flint does something of a hatchet job on the revered Lord Lugard (he allows that Lugard became a better person after retiring from Nigeria, which is when his more admiring biographer, Margery Perham, came to know him); Duignan gets a bit carried away with Sir Robert Cornynndon (a companion of Cecil Rhodes already!) but supplies enough information so that the defense of some controversial moments in the man's Uganda career seems reasonable, if awkwardly overstated.

The section on French governors by Leland Barrows, Henri Brunschwig, William Cohen, G. Wesley Johnson, and Virgil Matthew is perhaps the most informative since relatively little serious work has previously been done on the running of this colonial system (except for Brian Weinstein's biography of Eboué, which is condensed into an article here). The British section has a very lengthy introduction by A. H. M. Kirk-Green that effectively draws upon the rich material available in print to assemble a valuable and entertaining portrait of the gubernatorial phenomenon. Harry Gailey shows why Sir Hugh Clifford was really better than Lugard, and Ronald Robinson's brief essay on Sir Andrew Cohen breaks new ground on the final period of the British African empire, while whetting the appetite for a promised full-length biography. Both the introduction and the two biographies in the Portuguese section are by Douglas Wheeler, and again the information is quite new and valuable, even if it focuses more on how fascists and liberal nationalists thought about African territories than the manner in which they were actually governed. The Belgian and German sections are also brief; in the latter, Gann contributes an account of Heinrich Schnee which is nicely balanced between sympathy and criticism and comes perhaps closest of all the pieces in the book to presenting a typical figure.

Despite its shortcomings, therefore, this volume makes a valuable addition to the Gann and Duignan colonial history opus. It is not my idea of a

model for further work in this field, but, with Gann and Duignan themselves still going strong, that is not really an issue.

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TERENCE WALZ. *Trade between Egypt and Bilād as-Sūdān, 1700–1820*. (Textes Arabes et Études Islamiques, number 8.) Cairo: Institut Français d'Archéologie Orientale. 1978. Pp. xvii, 297.

This monograph is the first archival study of the historic trade route between Egypt and the Sudan. Based on the records of the Maḥākīm al-Shar'īya (Cairo), the Archives of the Ministry of Awqāf (Cairo), the French Archives Nationales, the British Public Records Office, and the National Archives at Khartoum, it resembles the pioneering work of André Raymond, *Artisans et commerçants au Caire au XVIII^{ème} siècle*, in its use of sources, focus on commodity prices, and the layout and structure of the Wakāla among the markets of Cairo. It differs from Raymond's work in its more monographic approach to the Gallāba, merchants who traded in the goods of the Sudan. Terence Walz's account also serves as a corrective to the popular view of the slave market found in Gerard De Nerval's account. Walz suggests the essentially utilitarian character of the slave as a commodity along with the complex moral and legal situation that the institution of slavery represented in Egyptian life. Walz's material on the escape of some slaves, the personality traits of male and female slaves who asserted themselves, and some information about their origins is quite new. The discussion of the training of slaves, both men and women, in arts and crafts, industry, and skilled cooking shows how their value rose as their potential for economic activity increased. The trade of the Gallāba merchants was not composed solely of slaves; diversification was an economic necessity. Ivory, ostrich feathers, and many other commodities were also traded.

In 1800 French officials collected taxes directly from the Wakāla for the central treasury, abolishing the middlemen of the Ottoman period. Later Muhammad 'Alī made his government the official purchaser and salesman of commodities. The number of products that the Wakāla traded shrank under this system of the Nāẓir Maṣlaḥat al-Wakālat al-Gallāba. By the middle of the nineteenth century the Sudan trade favored rich Greek merchants who went into partnership with the Assūyṭī's, bringing to an end the Wakāla in Cairo as an important institution.

The use of the trade route as a unit of analysis reveals how the Cairo merchants owned property

along their route in cities like Asyūt. The archives make clear that a wide range of "nationalities" were involved in this trade. The lives of three shaykhs of the guild further demonstrates the multisided activities of the merchants—as administrators, fiscal authorities, military figures, and labor negotiators with craftsmen. It is difficult to subsume all this under the concept of merchant or merchant capital, the concept in general use. What is now becoming apparent is the need for a new formulation. This comes out also in a consideration of the diversity of merchant nationalities; many came from nomadic areas such as oases. Perhaps these were neither merchants nor different nationalities but capitalists engaged in commercial and industrial production, and caught up in a labor history. The capitalists whose assets are listed in the archives needed nomads to carry their goods and artisans to fashion them for various markets. In all industrial contexts there is struggle. In this instance, presumably, sometimes the nomads had power and sometimes the artisans.

PETER GRAN
Temple University

PETER GRAN. *Islamic Roots of Capitalism: Egypt, 1760–1840*. (Modern Middle East Series, number 4.) Foreword by AFAF LUTFI AL-SAYYID MARSOT. Austin: University of Texas Press. 1979. Pp. xvii, 278.

This is an important and stimulating book that challenges the ethnocentric notion that modernist thought and a capitalist economy could only be transferred to the peripheral states through direct contact with the European center. Peter Gran argues that the capitalist transformation of the Egyptian economy was begun by Muslim merchants and Mamluk rulers in the eighteenth century. Those chapters dealing with the cultural, mainly intellectual, underpinnings of this economic transformation are the most outstanding. Gran follows the career and writings of Shaykh Ḥasan al-ʿAttār (ca. 1766–1835) to demonstrate that the evolution of modernist concepts among the religious class in Egypt resulted from indigenous developments and was derived from classical Islamic sources. He reviews an impressive range of religious and literary works to demolish the previously held view that the eighteenth century was a period of economic decline and cultural stagnation.

The study is divided into two general periods that he sees as dominated by merchant capitalism (1760–90) and state capitalism (1790–1840). According to Gran, the economic structure had a direct impact upon the religious thought of these periods. "For the period in which merchant capital predominated, the modality of logic was induction, which was rooted in ḥadīth; for the period of state

commercial dominance systematic theology rooted in deductive logic enjoyed a rebirth" (p. 179). Herein lies the major weakness of this study. Gran has produced an outstanding intellectual survey from sources almost totally unread by Western scholars, but he stretches his literary sources too far and ties his theories to an ill-defined framework of a world market economy. Without consulting Egyptian, Turkish, or European archival materials he constructs broad theories of social and economic history, weaving into his theories an unusual number of dichotomous propositions and sweeping generalizations that are sure to be challenged, especially since they too often seem to place events in strict causal relationships. He argues, for instance, that "Aristotelian logic was at a low ebb in Egypt during the period of commercial predominance. It recovered in the early nineteenth century as the state took over the capitalist sector and subsumed it under its administration. Its recovery occurred as the actual social relationships shifted to the polarity of bureaucrat and peasant" (p. 50).

Few of Gran's conclusions will go unchallenged; some are unfounded. He implies that the Wahhabi emergence in central Arabia resulted solely from the decline of the Gujerat–Red Sea trade (pp. 101–02); asserts that the influx of European goods destroyed the guild structure by the end of the eighteenth century (p. 178), despite published work that demonstrates its survival throughout the nineteenth century (for example, Gabriel Baer, *Egyptian Guilds in Modern Times* [1964], which is not included in the bibliography); and argues that the religious leaders consciously promoted a Sufi revival among the masses as a means of stabilizing a turbulent social situation among the artisan class (p. 179).

Gran's book, required reading for students of modern Middle Eastern history, is a pioneering study into the intellectual and economic history of Egypt. Its basic contentions and sweeping conclusions are sure to provoke further debate and stimulate research.

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J. DEAN O'DONNELL, JR. *Lavignerie in Tunisia: The Interplay of Imperialist and Missionary*. Athens: University of Georgia Press. 1979. Pp. xvii, 300. \$17.00.

Although this book concentrates on Lavignerie's work in Algeria and Tunisia it also treats his entire ecclesiastical career from his leadership of the *Oeuvre des Écoles d'Orient* in 1856 in the eastern Mediterranean until his death in Algiers in 1892. The question of whether the love of the Church or French patriotism drove this energetic and ambitious man is never fully answered. The two forces were so

strong in his life that their influence is inseparable. In 1868 Charles-Martial Allemand-Lavigerie founded his Society of Missionaries of Algiers, the White Fathers, which became the strongest French missionary force in Africa. His ignorance of and prejudice against the Muslim faith was matched only by the zeal he put into the early mission of the White Fathers to convert North Africans. Lavigerie eventually understood that numerous conversions would not occur and he then had the mission concentrate on serving European Catholics and establishing schools and hospitals that also ministered to Muslims. As archbishop of Algiers Lavigerie worked diligently, starting in 1875, for the extension of French control over Tunisia. Since Louis IX of France (St. Louis) died in Carthage in 1270 while on a crusade, Lavigerie sought to have the see of Carthage restored with himself as bishop.

This book, using an abundant supply of primary sources, shows how Lavigerie was successful in the dual function of extending French control over Tunisia and becoming the archbishop of the archdiocese of Carthage, at Tunis. He also became the political confidant of the French consul general at Tunis, Théodore Roustan, and of Paul Cambon, who arrived as resident-minister in 1882. The book gives only slight consideration to the major changes necessary to give France real control in Tunisia—the abolition of capitulations and consular jurisdiction. This was almost solely the work of Paul Cambon and not of Lavigerie. The book gives the impression that almost everything depended on Lavigerie and it ignores the work of the highly competent Cambon. This is, however, the only weakness in a very valuable study of Cardinal Lavigerie (he received the red hat in 1882). It is also a scholarly contribution to the history of Tunisia. The mark that Lavigerie made on Tunisia and a fitting summary of his life is provided in Resident-General Rouvier's statement on Lavigerie's successor: "We need here a prelate with a French heart, who is a politician more than an apostle" (p. 196).

Lavigerie and Cambon each established a style in their respective offices in Tunisia that their successors could not match. Today, the Basilica Saint-Louis rises out of the Tunisian landscape as mute testimony to the work of Cardinal Lavigerie.

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JOHN VOGT. *Portuguese Rule on the Gold Coast, 1469–1682*. Athens: University of Georgia Press. 1979. Pp. xii, 266. \$18.00.

In 1482 the Portuguese built a fortress on the Gold Coast (modern Ghana), situated about eighty miles southwest of Accra. They called it "São Jorge da Mina" (Saint George of the Mine)—the "mine" being a wistful reference to the gold fields they were

never able to approach—but in recent centuries Europeans have referred to the place as "Elmina." The Portuguese traded at that location until the fortress was captured by the Dutch in 1637.

John Vogt's book tells the full story of Portuguese Mina for the first time, exhausting most aspects of the topic. This straightforward narrative monograph can be divided into four phases: (1) preliminary exploration and the building and organization of the fortress-factory (1434–82); (2) successful commercial operations (1482–1580); (3) declining commerce and challenges from the Dutch, French, and English (1580–1637); and (4) fall of Mina and eviction of the Portuguese (1637–82).

The European side of the story—based on archival research in Portugal, Spain, the Netherlands, Britain, and Italy—is told fully, but the African side, unfortunately, needs more imaginative treatment and further research. Interesting details include the Portuguese use of oared galleys to defend the coast, and the curious "backwards" slave trade, from the island of São Tomé to Mina, which involved slaves that probably originated in Congo-Angola.

"Appendix C" contains a table of Mina gold receipts at Lisbon for forty-eight scattered years during the period 1487–1572. They seem to indicate an annual average of from 450 to 500 kilograms during the years 1490–1520, falling to 150 kilograms per year after 1550. All the figures given add up to about 16,000 kilograms of gold—an amount that at the summer 1979 gold price of \$300 per ounce would be worth over \$150 million.

Vogt does not do much with his figures, but one can extrapolate from them to arrive at an estimate of total Portuguese gold extraction of 33 tons from Mina during the hundred-year period 1487–1586. This was enough gold to keep Europeans interested in Mina, yet represented only about 5 percent of the total world production of about 640 tons per century. And the figure of 33 tons pales beside the estimated 225 tons of gold extracted by the Spaniards from the Americas during the sixteenth century.

The book has a reproduction of a sixteenth-century sketch of Mina and also a map of the coast and a ground plan of the fortress. "Appendix B" lists the names of forty-eight governors of Mina, many of whom also served in India and Brazil.

It should be noted that the expulsion of the Portuguese from the Gold Coast in 1682 was not permanent. Hundreds of Portuguese and Brazilians, as well as Afro-Portuguese, traded there in the eighteenth and nineteenth centuries. Only in 1961, when the Republic of Dahomey was established, were the Portuguese forcibly evicted from their last fortress, the quaint compound of São João Baptista de Ajudá at Ouidah.

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RHODA HOWARD. *Colonialism and Underdevelopment in Ghana*. New York: Africana Publishing. 1978. Pp. 244. \$22.50.

The relation of colonialism to underdevelopment has become a prominent theme in recent works on African nations. Walter Rodney in *How Europe Underdeveloped Africa* (1972) and Chinweizu in his historical overview, *The West and the Rest of Us* (1975), have presented the theme most fully for Africa south of the Sahara. In the volume under review Rhoda Howard treats this theme to some extent in her postscript and in more specific detail for the Gold Coast (referred to as Ghana) from 1885 to 1939. The effects of colonial rule are analyzed along with the development of what is termed the peripheral capitalist economy and economic oligopolization. The formation of a new class system in the African sector is also traced in this new economy (p. 181). The author uses neo-Marxist "dependency theory" for making this analysis. Her primary sources are drawn from colonial documents and business records such as those of the Manchester Chamber of Commerce.

It would have been helpful if several aspects of the presentation had been clarified further so that the historical facts could be better interpreted by the reader. For example, when mentioning British involvement in economic activities on the West African coast in the seventeenth century (p. 29), a brief summary of the various activities undertaken by the British through each century mentioned would have been useful. Only brief attention is paid to the civil servants who came to Africa from Britain (p. 154), or, for that matter, from many other parts of the Commonwealth. Again the author should have indicated the period to which she refers: is it the 1920s or the 1930s? Also, some attention should have been given to the educational background of the so-called new poor.

Further, more detail could have been provided about the implementation of the 1937 agreement among various firms, together with more discussion about the types of recommendations that came from the Colonial Office as indicated in the Nowell Commission Report (pp. 108, 200). At the very least, reference should have been made to works that give these details (for example, my article on the 1930 Gold Coast crisis and 1977 volume on British business and Ghanaian independence). Mention of other sources on the effects of transferring banking institutions to the Gold Coast would also have been helpful (for example, C. Brown's 1965 seminar at Birmingham).

Howard elaborates on the colonial underdevelopment theme. One wonders though, if as a result of these economic trends, there were not some by-products that were or could have been used in the development of the economy for this new independent state. Certainly such aspects as a distribu-

tion system, a Western-educated elite, and business-oriented managers drawn from the indigenous population should be considered.

After the war years (1939-45), with the British Labour government's emphasis on development toward independence, the people of the Gold Coast moved quickly toward statehood. In her postscript Howard emphasizes continuance of the colonial underdevelopment theme for these post-World War II years, as did Bjorn Beckman in *Organising the Farmers* (1976) and R. Green and A. Seidman in *Unity or Poverty?* (1968). It is of course true that in this period economic and political systems throughout the world became more complex and interdependent. But are Marxist- or capitalist-derived characterizations sufficient to explain these complex new systems? Such characterizations may be useful in beginning an analysis, but they should not be regarded as providing full interpretations. In both the developed and developing nations today, internal governmental controls and international aids and controls make theoretical interpretations difficult. Perhaps our measure in evaluating governmental and economic development could be the degree to which government and business leaders are concerned with the welfare of the people as a whole rather than their own selfish interests. Particularly in the late 1970s Ghana has been ruled by military governments that have been concerned with their own interests. This situation has led to breakdowns in both the economic and political systems. These aspects are not covered by the Howard postscript. Chinweizu's advice "to demythify the conflict, pierce through all that ideological propaganda the West uses to screen off the real issue, and focus on the politics of world resources use" (p. 481) should be heeded.

JOSEPHINE F. MILBURN
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ARTHUR ABRAHAM. *Mende Government and Politics under Colonial Rule: A Historical Study of Political Change in Sierra Leone, 1890-1937*. Freetown: Sierra Leone University Press; distributed by Oxford University Press, New York. 1978. Pp. xiv, 330. \$22.00.

Recent historical work on Sierra Leone, as elsewhere in West Africa, has emphasized how the fluid and frequently innovative political institutions of precolonial states were often stultified in their development by the imposition of colonial rule. This argumentative book by a young Sierra Leonean historian, Arthur Abraham, examines the combination of force and guile by which the British imposed a protectorate over the Mende chiefs of southern Sierra Leone and then "fossilized" the Mende pattern of government into a fragmented mass of petty chiefdoms readily manipulated by the colonial administration. In doing so, he offers new details to substantiate the general "revisionist" approach, as

well as some challenging speculations that should stimulate further work.

Some of Abraham's major claims would spark little controversy. He demonstrates, for example, that domestic "slavery," which furnished one pretext for British intervention in Mendeland, was a very benign form of clientage, hardly worthy of abolitionists' fervor. His evidence makes clear that the main pressure for annexation of the Sierra Leone hinterland came from the Sierra Leonean traders, with more fitful support from colonial officials on the spot and fairly consistent, if irresolute, opposition from the Colonial Office. He acknowledges, too, that among British officials and politicians there was some concern over the duplicity and the brutality shown by some British representatives. He also neatly turns upside down earlier "modernization" arguments by demonstrating that, in making Mende chieftaincy ascriptive and insulating it from pressures from below, the British transformed it from a relatively "modern" to an "antiquated" institution.

His most important claim, however, is also his shakiest. Abraham contends that Britain deliberately fragmented Mende political institutions by reducing the position of "kings" of Mendeland's major states to the same status as their erstwhile subordinate "chiefs," thus allowing local decentralizing tendencies full play. Unfortunately, he never demonstrates that these "kings" enjoyed any meaningful power over their "states," except in those transitory situations where an effective warrior-administrator like Kailondo could subdue other chiefs during his lifetime. The heads of the five "territorial states" he perceives along the coast enjoyed symbolic superiority over other chiefs in their area, but there is no evidence to suggest such a "king" could compel these other chiefs or their subjects to do anything on his behalf. The "personal-amorphous states" of the interior, on Abraham's own admission, owed their being to the power of a particular leader, and it seems improbable that any would have survived that leader's death even if no colonial power had ever intervened.

Still, despite the weakness of its main contention and the fact that it is not quite as original as its author claims, this book is useful in providing us with further details on the workings of British colonialism.

JOHN R. CARTWRIGHT
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M. G. SMITH. *The Affairs of Daura*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xix, 532. \$30.00.

In 1804 in the area which later became northern Nigeria, the declaration of a *jihad* marked the cul-

mination of a religious reform movement and the beginning of successful military campaigns that toppled most of the ruling Hausa dynasties and created the Sokoto Caliphate, a loose federation of emirates headed by Fulani conquerors who owed allegiance and tribute to the successors of the original reformer. Historians have used Arabic correspondence and other writings of the Fulani aristocracy (as well as a smaller volume of material in Hausa and Fulani), the accounts of travelers, and oral data to describe the government and military affairs of the caliphate and individual emirates. Most recently studies of economic history, the sociology of warfare, and slavery have been in the forefront.

M. G. Smith's book returns to issues first raised in his own pioneering work on government in Zaria and shows how much can be learned from the study of these precolonial states. The book is the first in a series that will use Smith's field work of 1958-59 to trace the political history of the Fulani emirates as well as of Hausa states that successfully resisted Fulani conquest. The emirate of Daura was a wise choice for the launching of the new series. A Hausa government in exile survived there, and Smith can therefore compare four variations of precolonial government: Daura before the Fulani *jihad*, the Hausa successor state, one of two Hausa splinter groups, and the state formed by the Fulani conquerors. The book begins with introductory material and a chapter describing pre-*jihad* Daura government, the form of which persisted in the three other states under study. That government was characterized by a differentiated structure of status; offices as the principal units of government distinguished from each other by rank, title, material and social resources, traditions, rights, privileges, obligations, and relations to other units; chiefship as the most senior office; electoral councils; territorial administration through fiefs; and an administrative apparatus in which senior officials had staffs of titled assistants. The next chapter presents a minutely detailed political history of the main Hausa successor state, Zongo. Smith describes its successive schisms, its waning military fortunes and subjugation to a suzerain state, and the subversion of the power of the chief by the *kaura*—one of the two senior titled officials—with the collaboration of the suzerain ruler. The following chapters treat the Fulani government, one of the two Hausa splinter states, and restoration Daura under British colonial rule, which began in 1903. Not suspecting that the *kaura* was predominant in Zongo and choosing to ignore that he ruled half the kingdom, the British restored in one stroke the power that the chief had lost over several decades. Smith devotes the last three chapters to an analysis of the political narrative, first examining each important event by using an abstract scheme that consists of the latent cate-

gories in local government. He then uses corpora-tion theory for a more concrete appraisal of change in the state, the government, and the chiefship.

Minor criticisms scarcely detract from this major achievement. Smith might have given readers more information as a basis to judge his use of oral data, his most important source material; but he inspires trust because of what he says about his method and because of his clear reasoning in areas where he is forced to speculate. Even the long delay in publication can be forgiven in view of the complexity of the task—Smith's interest in generalizing about political change—and the time he lost having the work translated and read to his principal Daura informants before publication.

STEPHEN BAIER
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JAN S. HOGENDORN. *Nigerian Groundnut Exports: Origins and Early Development*. (Ahmadu Bello University History Series.) Zaria: Ahmadu Bello University Press and Oxford University Press, Nigeria. 1978. Pp. xvi, 173. \$29.95.

Here is another important chapter in the history of *homo economicus* in rural West Africa. Complementing earlier studies of the rapid and rational responses of West African farmers to international marketing opportunities in palm oils and cocoa, Jan S. Hogendorn's splendid little book examines the peanut export boom from northern Nigeria in the early colonial period. The focus is on Kano province, from which most of the commodity came, during the two years after the Lagos-Kano railroad was opened to general traffic in April 1912. While the emphasis is on the Hausa farmers and traders of the region, the role of European merchants and consumers is also carefully presented. An epilogue usefully sketches the expansion of the trade down to the present.

The railroad's opening coincided with a boom in European use of margarine and the development of the process of hydrogenation of soft oils, creating a demand that drove the price of shelled peanuts in Kano from £5 a ton to £10 in the first season. The reaction of the Hausa, whom Europeans ironically had considered unresponsive to market forces to the point of "combative inertia," was dramatic. Peanut exports from Kano alone went from near zero to some ten thousand tons in 1912-13, clogging the railroad for months after the actual harvest, and Hausa farmers planted even more heavily for the next season. While the Hausa accurately foresaw the railroad's potential, the peanut boom, Hogendorn demonstrates, surprised almost all European officials and merchants. The British Cotton Growing Association and British colonial officials had been actively promoting the railroad in anticipation

of a boom in cotton that, in reality, never materialized. Hogendorn further argues that the significance and, indeed, the existence of this massive economic response by Hausa farmers and middlemen has not been appreciated because the exports in the subsequent two years were curtailed by extraordinary acts of man and nature. In 1913 the crop was decimated by one of the worst droughts on record, though peanut exports still climbed 16 percent above the previous season. Export of the 1914 crop became impossible when the opening of World War I cut off German shipping and markets. Once these impediments were removed, Nigerian exports soared to some fifty thousand tons a year in 1916-21 and eventually to a peak of eight hundred and seventy-two thousand tons in 1962-63.

Because Europeans had not foreseen the early peanut boom, the factors promoting African production and marketing went almost entirely unrecorded at the time. For this reason the most significant and original part of Hogendorn's work has been the reconstructing, through extensive oral interviews conducted in 1965 and 1975, of the ways in which Hausa farmers and traders had perceived this economic opportunity and responded to it. By combining this information about farming methods, land tenure, credit systems, and middlemen very skillfully with European colonial and commercial records, he has produced a detailed and definitive account of the beginnings of a major commodity trade. The material is presented with the rigorous precision of a trained economist and the sensitivity of one well familiar with the region. In short, Hogendorn has produced a superior monograph, meticulously researched, skillfully analyzed, and narrated with a lucidity, brevity, and grace that make reading it a delight.

DAVID NORTHRUP
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JOHN TOSH. *Clan Leaders and Colonial Chiefs in Lango: The Political History of an East African Stateless Society, c. 1800-1939*. (Oxford Studies in African Affairs.) Oxford: Clarendon Press. 1978. Pp. xi, 293. \$29.95.

This book presents a discussion and analysis of the structure of Lango political authority and its transformation over a century and a half since 1800. It is a welcome addition to a growing number of monographs on stateless and small-scaled societies in Africa (for example, J. Lamphear, *The Traditional History of the Jie* [1976] and J. B. Webster, *The Central Lwo during the Aconya* [1976], and helps to fill a void in the literature, which to date has too frequently discussed hierarchical state systems. John Tosh's volume is also important in another respect. He focuses on both the precolonial and colonial eras, thereby providing a useful comparative perspective

about the changing nature of political authority in an African society. Until now, too many studies have limited their investigation to one era or the other.

Tosh initiates his study with a valuable introduction on the "Stateless Society and the Historian in Africa." He reviews the place of stateless societies in African historical literature and capsulizes the conflicting viewpoints of anthropologists and historians. Until recently, the central cause for historians' neglect of stateless societies has been the debate concerning whether their oral traditions could be collected and evaluated. Tosh's work is an affirmation that they can be. He used oral interviews of Langi clan members and corroborated this material with colonial and other written records. Several examples of the transcribed interviews and a useful methodological statement on his fieldwork are included in two appendixes.

Lango society, like others in northern Uganda and East Africa, was shaped by the patterns of migration and settlement, in this case primarily during the nineteenth century. For most of the period Lango political communities remained amorphous, controlled by clan and lineage leaders. The first four chapters of the book explore the evolution of this type of leadership pattern, its values, its weaknesses, its major personalities, and its tendencies for change prior to the appearance of British interests in the area in the 1890s. British rule imposed for the first time a centralized political system on the Langi. The transition of Lango political patterns to conform with British perspectives on administrative efficiency resulted in the evolution of the infamous "colonial chief" in a society that had no traditions of such focused authority.

The last four chapters provide an excellent discussion of the manipulations, frustrations, resistance, and restructuring of local politics during colonial rule. Particularly well elaborated are the details on how local families and individuals maneuvered and countermaneuvered within the colonial system to achieve dividends, either by calculated resistance or collaboration.

As a sharply focused study of political change, the work contains few remarks about the concurrent economic and social change occurring in Lango society. Tosh does mention the impact of trade on early leadership patterns and discusses briefly the changing agricultural routines, the imposition of labor demands, and the impact of missionary education during the colonial period. Since these themes are not central to his thesis, he presumably leaves the reader to consult his other writings on some of these subjects (for example, "Lango Agriculture during the Early Colonial Period," *Journal of African History*, 19 [1978]).

In all, the book is a well-written and illuminating case study that helps to corroborate the important

perspective that African interests were central and in many ways shaped the process of colonial rule. By detailing the historical dimensions of Langi political values and actions as they faced a changing world, the book helps to remove the bias of colonial history from such evaluations.

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NORMAN R. BENNETT. *A History of the Arab State of Zanzibar*. (Studies in African History, number 16.) London: Methuen. 1978. Pp. 304. \$19.50.

With the active encouragement of its rulers, Zanzibar in the nineteenth century became the major entrepôt of the western Indian Ocean, servicing intensified world commerce and linking it with an ever-widening mainland network of trade in East Africa. Beginning in 1890, a British colonial administration reduced the domain and fortunes of the Busaidi dynasty and stabilized the island society by protecting the "Arab" owners of clove and coconut plantations. This class formed a local oligarchy supported by access to education, manipulation of labor to reduce its costs, and restraint on Indian creditors. A revolution of workers and lower classes against the sultan and the constitution took place shortly after the country achieved independence in 1963. The history of Zanzibar during the period covered by Norman Bennett's book is indeed epic, easily focused, and yet multifaceted.

What a disappointment it is, therefore, to find the rendering so narrow. The strength of the work, its close adherence to documentary sources, dissolves into weakness when the limitations of these materials become apparent. Much of the same evidence has been used in previous studies of the nineteenth century, most notably by Reginald Coupland, the Oxford scholar who wrote during the interwar period. Bennett, like Coupland, emphasizes rulers and rival foreign interests, but he goes further than Coupland's Anglocentric classic to consult American and French sources. The Coupland tradition, modernized by John Flint in his chapter on Zanzibar from 1890 to 1950 in the second volume of the *Oxford History of East Africa*, still has qualities of insight, sustained argument, and literary style that are singularly lacking here. How can the Methuen editors have been so indifferent? The main point, however, is neither the viability of the older scholarly tradition nor the inadequacy of style. What is most wanted is some reflection of recent historiography, with its concerns for the processes and ramifications of Western economic penetration as the context for politics. While the contributions of Abdul Sheriff and Fred Cooper are mentioned, the discussion does not grapple with the issues they have raised.

The leading components of the colonial situation are indicated without being brought into conjunction. Stress is laid on the varying fortunes of the clove industry and the mistake of excluding the Swahili elements of the population from educational opportunities. Yet, with respect to the latter, we learn that when these groups were offered schooling, they did not respond positively. Why? Insufficient attention is given to the mainland-derived workforce, the fluctuating commercial and port conditions in urban Zanzibar, and the rigidification of social and racial stereotypes, all of which were preconditions for the revolution of 1964.

A comparable one-volume history of Zanzibar before 1964 is not available. A gap has therefore been occupied, if not filled, and students have at least a point of departure. The unfinished task of interpretation remains. Those who are curious and investigative can go in many fruitful directions, but they must above all be propelled by the question that eludes the present author: "why?"

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BISMARCK U. MWANSASU and CRANFORD PRATT, editors.
Towards Socialism in Tanzania. Buffalo: University of Toronto Press. 1979. Pp. x, 243. \$20.00.

The development of public policy in Tanzania, presided over by the eloquent Julius Nyerere, has been of considerable interest to those concerned with modern African political evolution. In 1967 Tanzania's only party, the Tanzania African National Union (TANU), committed itself to achieving socialism. With most citizens engaged in subsistence agriculture, a severe shortage of skilled technicians and management personnel, and a clear certainty that rapid economic development was impossible, Tanzania appeared an unlikely locale for the success of any policy designed to ameliorate the living conditions of most of its population. Nonetheless, Tanzania's approach calls for careful observation since TANU, in contrast to most other African parties striving for progress toward some form of socialism, planned to manage the transition to full socialism without coercing the country's peasants and workers to follow foreign doctrines imposed by a narrow ruling elite. In 1976 a conference held at the University of Toronto considered the Tanzanian experience; this volume, with additions from subsequent discussions, presents a selection of these deliberations.

The editors open with a useful introduction, supplying the necessary background and the current state of the debate on the success or failure of Tanzania as a society in transition to socialism. R. H. Green continues with an investigation of goals,

strategies, and results from 1967 to 1974, his analysis presenting a clear description, with all mistakes and victories, of Tanzania's step-by-step implementation of policies. Green, along with several other contributors, identifies the most positive fact in the experience: the need for the government to convince peasants and workers that it is moving to meet their requirements. So far, it appears, this criterion has been achieved. I. Parker contributes by recounting the problems involved in realizing TANU's goals in one important organization, the National Development Corporation; his article is an informative effort except when he abandons a historical framework to plunge into policy recommendations. J. Loxley adds valuable analysis in considering monetary institutions and the class struggle; he among other matters elaborates upon the state of the Tanzanian bureaucracy, finally concluding that this group—so vital in every African country—is reasonably open to policies outside its own narrow interests. J. Barker and J. Boesen examine the most striking of Tanzania's initiatives, the effort to create a rural socialism, first through *ujamaa* villages and then through villagization, the latter a process requiring the moving of all Tanzanians into villages by 1976. Both authors show the many difficulties encountered in this major social transformation, with Boesen—to me the most perceptive contributor—offering ample evidence of how the government violated the ideals expressed through its leaders by coercing individual citizens into following decisions imposed from above. C. Pratt closes the volume with an intelligent piece in which he presents the views of critics of the Tanzanian experience, especially those he designates as "Marxist socialists," individuals who conclude that Tanzania is not in transition to socialism. In contrast, as an avowed "democratic socialist," Pratt contends that Tanzania, with a political party open to debate and with some form of elections to sample the opinions of the masses, is on the path to socialism. He recognizes that both democracy and socialism may not triumph in Tanzania but maintains that the path for both is still open.

Whatever the reader's predilections on the path of contemporary political development in Tanzania, and the remainder of Africa, this volume should provide considerable matter for reflection. Those interested in the emergence of democratic societies (in the Western sense) will find much of value to contrast with the political realities of life elsewhere in Africa.

NORMAN R. BENNETT
Boston University

ROBERT V. KUBICEK. *Economic Imperialism in Theory and Practice: The Case of South African Gold Mining Finance*,

1886-1914. (Duke University Center for Commonwealth and Comparative Studies, number 45.) Durham, N. C.: Duke University Press. 1979. Pp. xii, 239. \$12.75.

For three generations debate about modern imperialism has revolved around theories of its economic or capitalist roots and determinants. South Africa has been taken as the classic case. To the critique of this case Robert V. Kubicek offers an interesting and original contribution. He agrees with those who assert that all interpretations are based on inadequate evidence of the structure and behavior of the business interests involved. The fundamental questions still need answering. Who, and what, were the controllers of the mines? What were the bases of their operations? What were their business strategies and how was capital raised and used? Previous studies are especially weak on these points. The special merit of this book is its use of new evidence in the mine controllers' records, especially those of the great Corner House group, but also of Consolidated Goldfields and numerous smaller operators. Much has been known, of course, but—and this is new—the result is a probing analysis that supplies unprecedented and extensive detail on financial sources and mechanisms, priorities and strategies, similarities and differences among groups, companies, and individuals. Its weakness lies in a relative lack of balancing treatment regarding political connections, but part of the argument apparently rests on the absence or fecklessness of such activities. Kubicek is "struck not by the forcefulness but rather the restraint with which the magnates used their economic power" (p. 199).

His findings are destructive of economic determinist and neo-Marxist interpretations. Finance capital was fragmented, self-destructively competitive, and politically feeble. Absentee, international, non-British interests and perspectives tended to dominate. Business strategies were incompatible with government priorities. Rhodes, the outstanding local entrepreneur, was not prototypical: no other major magnate shared his intensely political priorities or boldness. Most were interested primarily in speculation, and much less capital was actually invested than was raised in promotion. Huge amounts went to safe government securities, expatriated personal fortunes, and ventures in other countries. It is well known that in the mid-1890s big German and French capital was interested in direct control, but the competition and irresponsible speculation of the mine controllers, political turmoil, and the onset of the war turned them away. Some mines were immensely profitable, but even the greatest groups, Corner House and Goldfields, were attempting to withdraw and were investing outside the empire. The last thing wanted was war and

British takeover. In the decade before 1914, big German, French, and British finance was retreating from South African gold mining.

This book offers neither a simple refutation of economic determinism nor unqualified support for its major alternative inspired by the well-known work of Robinson and Gallagher. Rather it adds qualifying complexity to the debate. "International capitalism, British imperialism, and Afrikaner nationalism did to some extent coexist. But these forces . . . were fundamentally at cross-purposes. South African developments, consequently, should be seen basically as a function of clashing priorities and the inability of any one or a combination of these forces to achieve supremacy" (p. 204).

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ASIA AND THE EAST

J. ARTHUR LOWER. *Ocean of Destiny: A Concise History of the North Pacific, 1500-1978*. Vancouver: University of British Columbia Press. 1978. Pp. xiv, 242. \$16.50.

The Pacific Ocean, "the ocean of destiny," has long "presented a physical and psychological barrier separating North Americans from the alien civilizations of Asia . . . [but] the Pacific also is a bridge" (p. xiii). J. Arthur Lower attempts to cross the bridge as a Canadian, outlining Canada's connections with the history of the North Pacific.

Beginning with European exploration of the Pacific, Russian expansion into Siberia, and Spanish missionary imperialism in California, Lower examines the role of the fur traders and sealers, the missionaries and traders in establishing contacts across the Pacific and in penetrating China and Japan during the nineteenth century. During the "decisive decade," 1840-50, the stage was set for an economic, social, and cultural revolution in East Asia. New power conflicts developed as both North America and Europe sought to build formal or informal empires in this region. The "open door" and the Russo-Japanese War were important landmarks in this process. The emergence of Japan as a great power at the beginning of the twentieth century pointed the way to the undermining of European influence and then the gradual withdrawal of Europe between the two world wars. Despite the defeat of Japan in 1945, the era of European (and North American) domination of the western Pacific ended with the Korean armistice in 1953. A new multipolar balance was then established among

China and Japan, the Soviet Union and Korea, the United States and Canada.

The new multipolar balance is in fact quadrilateral rather than hexagonal: Canada and Korea are at present junior members. Whether a North-West Pacific community is developing is another matter. There are numerous bilateral economic and cultural agreements between members, but these fall far short of community ties; the European Economic Community and NATO are vastly different organizations.

Canada's role in the North Pacific has so far been marginal, since its foreign policy has been oriented toward Europe rather than Asia. Canadian participation in the Korean War was, in a sense, out of character, despite active participation in peace-keeping operations elsewhere; Canada, as a member of the ICC, "excused itself from direct involvement in the Vietnam War" (p. 172). Despite Lower's argument for an increasing Canadian role—scientific, cultural, and economic rather than military—in the North Pacific, he has to admit that in recent years it has become almost a forgotten region as "Canadians give a low priority to North Pacific affairs" (p. 205).

The view from Vancouver or Ottawa is not very different from that from San Francisco or Washington. It is lower in key, less concerned with power; policy is more passive than aggressive. As a middle power with a long common boundary with a superpower, Canada's freedom of maneuver is limited. "From the Asiatic point of view Canada is a friend, if not an ally or satellite, of the United States" (p. 175).

Lower has dealt very thoroughly and competently with the process of opening up the Pacific and East Asia. His book is very well illustrated and contains useful tables. The fine edge of his writing is blunted only in the last chapter, which tends to become a tract on modern Canadian interests in the Pacific. His range of sources is impressive, though it is a pity that he did not include the work of an important Canadian diplomat in his bibliography, E. H. Norman's *Japan's Emergence as a Modern Power*.

NORMAN HARPER

University of Melbourne

RAYMOND DAWSON. *The Chinese Experience*. New York: Charles Scribner's Sons. 1978. Pp. xxv, 318. \$25.00.

As a general introduction to traditional China, *The Chinese Experience* by Raymond Dawson is unique in three aspects. First, instead of giving a chronological account, it describes "the essence of the Chinese experience in all the major branches of human activity" (p. xv). As a result, the main body of the book is divided into four parts: Part one deals with the

political experience, part two with the philosophical experience, part three with the social and economic experience, and part four with the esthetic experience. Second, these facets of the Chinese experience are chosen and explained with a view to contrasting them with those of the Western experience. For example, as the author rightly observes, although religion colored all aspects of Chinese life, it never attained "that separate and independent status which a powerful church has sometimes achieved in European society" (p. 164). In the realm of art, it is important to know that the sense of movement in Chinese landscape painting required the Chinese artists to adopt a shifting perspective that is markedly different from the one-point perspective in European art (p. 212). There is also much truth in the observation that the Chinese language, characterized mainly by a "magnificent economy," is not the kind of vehicle of philosophical thought that "could have nurtured some of the problems which have preoccupied Western thinkers through the ages" (p. 236). Third, the Chinese experience is treated mainly as a living tradition. Certain traditional modes of thinking and patterns of behavior, for instance, are seen to be continually active in today's revolutionary China.

This last point is well taken in the author's discussion of the twentieth-century experience in his epilogue, where he shows great insight when he says, "The old-style mandarins who knew the Classics by heart have their modern equivalent in the cadres who are expert in the current orthodoxy. Very traditional, too, is the emphasis on political correctness to the detriment of technical expertise" (p. 288). The simple fact that the "redness versus expertness" controversy has been much more heated in post-1949 China than in any other socialist society clearly suggests that the problem must be deeply rooted in Chinese tradition. The author interprets this phenomenon in terms of the Confucian scheme of things, which always places "right thinking" above "technical concerns" (also see p. 19). Generally speaking, this is correct. It is also necessary, however, to point out that contemporary Chinese views on this matter have been sharply divided between those who stress the primacy of "redness" over "expertness" (*hsien-hung hou-chuan*) and those who embrace equally "redness" and "expertness" (*yu-hung yu chuan*). This division immediately reminds us of the Neo-Confucian rivalry between the Lu-Wang and the Ch'eng-Chu Schools. As we know, according to Lu Hsiang-shan (1139–93) and Wang Yang-ming (1472–1529), a man must set his mind morally right before he can meaningfully begin any kind of intellectual inquiry, whereas, according to Ch'eng I (1033–1108) and Chu Hsi (1130–1200), morality and knowledge are actually as inseparable as Siamese twins. Unfortunately, the

author says nothing about Neo-Confucianism and thus leaves half of the Chinese philosophical experience unaccounted for.

Equally unfortunate is the author's complete silence about the role of the prime minister(s) vis-à-vis that of the emperor in the Chinese political tradition. Almost from the beginning of the imperial age in 221 B.C., the relationship between the emperor as head of the state and the prime minister as head of the officialdom was characterized, more often than not, by tension and conflict that eventually culminated in the abolition of the prime ministership by the founding emperor of the Ming dynasty in 1380. The relevance of this historical background to the most recent Chinese political experience of the so-called Cultural Revolution is obvious. Is it too far-fetched to suggest that the role played by Chou En-lai is more reminiscent of that of a traditional prime minister under a founding emperor than that of a comrade-in-arms under a revolutionary leader?

YING-SHIH YU
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EVELYN SAKAKIDA RAWSKI. *Education and Popular Literacy in Ch'ing China*. (Michigan Studies in China.) Ann Arbor: University of Michigan Press. 1979. Pp. xii, 294. \$16.50.

Modern researchers of traditional Chinese society continue to assault the notion of elite and popular cultures as sharply differentiated and mutually impenetrable traditions. The book under review carries this discussion into an area that obviously is crucial to the issue: the distinctive Chinese ideographic script and educational efforts to master that script. Using evidence from Chinese local histories, clan genealogies, and school texts as well as a wide array of Chinese, Japanese, and Western secondary works, Evelyn Sakakida Rawski shows that in eighteenth- and nineteenth-century China there was a continuum from the barely literate to the highly educated scholar-elite class. Although the book focuses on Ch'ing China, the first and last chapters place education and literacy in the context of both traditional and contemporary China. The intervening chapters treat the types of education available, its cost and financing, the distribution of schools, popular literature and educational texts, and the role of literacy in Ch'ing society.

The author contends that the term literate should not be reserved just for those in traditional China who after long years of study mastered the Confucian Classics and moved into elite ruling circles, or at least attempted to. This fully literate elite was only the most visible end of a spectrum of literacy. There were also the educated merchants, shop-

keepers, and artisans in the urban areas, and even some landlords and rich peasants who could use their elementary literary skills to advance their careers. Below this level came functional literates who knew only a few hundred characters, but whose command of limited and often specialized vocabularies enabled them to keep records, read proclamations, and cope with the practical demands of daily life.

The bulk of the population was illiterate, with little or no recognition of the Chinese written script, but Rawski estimates that 30-45 percent of males and 2-10 percent of females possessed some level of literacy—functional, elementary, or advanced. The male rate is comparable to Japan's during the late nineteenth century and probably higher than in preindustrial Europe. Other variables, therefore, the author argues, must be used to explain Meiji Japan's rapid modernization and China's slower response.

The author has surveyed the educational materials used in elite and popular education, and again she finds that a continuum existed in the content and emphasis of these texts. At the popular level, there were arithmetic guides and character books (*tsa-tzu*). The latter ranged from simple illustrated glossaries (in which abstract ideas and Confucian concepts were ignored in favor of concrete objects and matters relevant to daily life) to collections of rhymed couplets forming meaningful phrases that began to exhibit elite concerns, to elaborate narratives for specialized audiences—such as merchants, prosperous suburban households, and children of middle-peasant families. As further aids, a person of limited education could consult a variety of popular encyclopedias.

In the elementary education provided by community and clan schools, whose numbers were increasing in the eighteenth and nineteenth centuries, three basic primers, the *Thousand-Character Classic*, the *Trimetrical Classic*, and the *Hundred Names*, introduced Chinese youth to China's past, its heroes, and Confucian precepts for proper conduct. The bulk of the school boys (girls rarely enrolled in these schools) received further Confucian values through popularized versions of the Classics. Boys from elite families preparing for the civil service examinations, however, were enrolled in formal studies with a tutor to tackle the central works of the Confucian orthodoxy, the Four Books and the Five Classics.

There are some organizational weaknesses to this study. The author has amassed a great array of details on the subject but occasionally presents it in a fragmentary fashion. This sense of disconnectedness is heightened in those chapters that have ten or more short sections. The concluding chapter, "Continuities in Modern Chinese Elementary Education," is more appropriately an epilogue; it does not

provide the reader with an adequate summary of the book's arguments. But these are somewhat picky observations. More importantly, the author has provided a new, richly detailed look at a key social institution of traditional China, the educational system in its elite and popular forms.

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THOMAS L. KENNEDY. *The Arms of Kiangnan: Modernization in the Chinese Ordnance Industry, 1860-1895*. (Studies of the East Asian Institute of Columbia University.) Boulder, Colo.: Westview Press. 1978. Pp. xvi, 246. \$17.00.

Was "self-strengthening" in nineteenth-century China a progressive movement revealing the resilience of the traditional order and its ability to innovate, or was it rather a conservative phenomenon that inhibited more fundamental responses? Although Thomas L. Kennedy's study of the fledgling Chinese ordnance industry does not explicitly ask this question, it is one that has inevitably informed most studies of modernization in late imperial China. Kennedy demonstrates that the self-strengthening movement went farther toward industrial modernization and had wider implications than has usually been assumed.

The narrative follows the development of modern arms production from its origins in the crisis of 1860, which brought home to leading Chinese statesmen the urgency of military modernization, to China's disastrous defeat by Japan in 1895. The initial efforts to supply arms for the armies engaged in suppressing the Taiping and Nien rebels were succeeded by the emergence of more permanent establishments when, after 1868, the orientation shifted to maritime defense against the Western powers. From 1868 to 1875 production of the three principal arsenals was dictated by the different perspectives and needs of their leading sponsors. The Kiangnan Arsenal, under Tseng Kuo-fan's influence, was oriented almost exclusively to steamship construction, while those under Li Hung-chang's tutelage were involved in ordnance production. From 1875 to 1885 a major reevaluation of policy culminated in the termination of shipbuilding and the development of ordnance, especially heavy artillery for coastal defense, at all three arsenals. Finally, after the experience of the Sino-French war in 1885, efforts were made to modernize the production and administration of the arsenals. (In two preliminary chapters, Kennedy discusses the development of ordnance production in traditional China before 1860 and the intellectual background of institutional reform and self-strengthening since the seventeenth century.)

Attempts to understand the promise or failure of self-strengthening have usually amounted to an examination of the political and social influences that retarded its development. Kennedy also takes this approach. The problems afflicting the ordnance industry included excessive costs of production and overhead, especially costs of material and personnel; difficulties in mobilizing and maintaining financial support; the nature of leadership and management; and foreign technological dependence. Perhaps the most important single influence was the decision, concluding the policy debate of 1872-75, to give priority to inner Asian frontier defense. Kennedy does not fully pursue the implications of this fateful decision, which pre-empted the emergence of a coordinated industrial modernization program. Although the arsenals made quite a respectable showing in arms production, it is questionable whether the foundations of a "military-industrial complex" (p. 18) ever existed outside the minds of a few Chinese proponents. The shortcomings of self-strengthening lay particularly in this failure to achieve a coordinated enterprise. Although the weakness of this monograph is that it stops short of probing the broader implications of self-strengthening and its failures, it nevertheless adds much to our knowledge of the movement.

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JING SU and LUO LUN. *Landlord and Labor in Late Imperial China: Case Studies from Shandong*. Translated with an introduction by ENDYMION WILKINSON. (Harvard East Asian Monographs, number 80.) Cambridge: Council on East Asian Studies, Harvard University; distributed by Harvard University Press. 1978. Pp. xiii, 310. \$15.00.

In the late 1950s Jing Su and Luo Lun of Shandong University wrote a monograph arguing that in the Qing (1644-1911) capitalist elements were emerging in rural north China. The key to this emergence was commercial landlordism. From conventional sources they showed that Shandong towns were growing, specialization increasing, and commercial agriculture expanding. In the countryside emerged an incipient trend toward a two-class society, a rural bourgeoisie exploiting a rural proletariat. Then they adduced some new sources: landlord account books and a survey of old-timers in one hundred ninety-seven villages. These sources showed that some "managerial landlords" flourished around 1900. Jing and Luo argued that this stratum must have been emerging in the Qing, an argument that squared with Mao's dictum that China had its own protocapitalist phase.

The study drew Japanese and Western attention in the 1960s. Its conclusions were not novel or fully acceptable. But the authors had focused on a little-

studied region, and they had generated some new data. Consequently, Endymion Wilkinson has translated it and supplied a critical introduction.

The following picture of Shandong emerges from the book. Rural and urban population grew steadily. The economy adapted through existing technology. Indeed, the society seemingly could not make full use of established techniques. Managerial landlords' yields far exceeded those of small land holders, but the landlords cultivated only a small fraction of Shandong's total arable land. And these efficient farms, pieced together over decades, sooner or later refragmented because of inheritance rules.

Shandong's rural inhabitants arrayed themselves on a finely graduated scale of wealth, status, and power. Some of the landless worked as long-term laborers for landowners, and some sharecropped. Some families owned a little land and rented a little. At a higher rung were families who owned enough to live on. All of the above might hire themselves out in peak seasons, and many pursued handicraft or peddling sidelines. Richer peasants might employ seasonal workers or even long-term labor, because their holdings were more than a family could farm. Then there were families who built upon rich peasant wealth, upon urban business money, or upon the spoils of officialdom and accumulated large holdings of one or two hundred acres, rarely as much as a thousand acres. Although not at China's social pinnacle, these families enjoyed local influence and sometimes purchased rank. The cream lived in the district capital and hobnobbed with officialdom.

Big landlords rented out their land to numerous small tenants. But some farmed directly fifty to a hundred acres. They supplied animals, tools, and hired labor. Part of the harvest fed and clothed their large households and their employees; part went to market. These families also operated handicraft, commercial, and financial enterprises.

Jing and Luo have presented new data on estate accumulation and dispersion, on the limits to managerial farm size, on the proportions of rented to directly managed land, on inputs and yields, on landlord backgrounds, and on employment conditions. Wilkinson's introduction supplies the context for assessing the study and points out some mistakes, excesses, and omissions. The whole is a worthy contribution to the study of traditional rural society.

CRAIG DIETRICH
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LUC KWANTEN. *Imperial Nomads: A History of Central Asia, 500–1500*. Philadelphia: University of Pennsylvania Press. 1979. Pp. xv, 352. \$25.00.

A new history of Central Asia, attractively packaged by a major university press, which aims to get

rid of old clichés and give us a new perspective on this key aspect of world history, incorporating all of the new scholarship that has been focused on this area in the more than three decades since the publication of René Grousset's *L'empire des steppes*, is surely an exciting prospect for a reviewer. Alas, my anticipations have been grievously disappointed.

The majority of Luc Kwanten's pages have to do with the Mongols, but he seeks to link this most spectacular and extensive of the steppe empires with the long tradition of similar eruptions that lay behind it. This is a sound idea, but hardly as original as he seems to imagine. He criticizes the "eventmental approach" that has allegedly prevented his predecessors from realizing that there was "an autochthonous historical tradition" in the steppe and that has led them to concentrate on charismatic personalities such as Chinggis Khan. Certainly there have been popular works of this kind, but the recurrent patterns of nomadic political formations and imperialistic expansion have not gone unnoticed or lacked attempts at explanation. The author's own attempts at providing continuity and a conceptual framework are not impressive. It takes more than a ritualistic invocation of the names of Fernand Braudel and the *Annales* school to give a structure to the chain of events and reveal their inner connections.

It is astonishing that the name of Owen Lattimore is never mentioned by the author, even in his bibliography. Although Lattimore's work is certainly in need of revision, his conception of the role of the frontier provides a far more sophisticated analysis of the cycles of political rise and decline on the steppe and their relation to economic and political cycles in the agricultural civilizations to the south than anything offered here.

Kwanten's narrative is not only weak in general conceptions but also highly unreliable in its factual information. Space will not permit a detailed listing of errors of this kind. A serious shortcoming, though, is the gross underestimation of the role of the oasis dwellers along the trade routes of Central Asia. One would scarcely guess, if one did not already know it, that when the Chinese first penetrated into this region toward the end of the second century B.C. they found a series of city states occupied by Indo-European speakers and that the struggle for control of these states and the trade routes that passed through them was an important aspect of the conflicts between the Han empire and the Hsiung-nu. The occupation of these oases by the Uighurs in the ninth century and the sedentary, literate civilization that developed therein are described, incredibly, without reference to the Tocharian Buddhist civilization that it supplanted and absorbed. Kwanten refers to the role of Sogdians in converting the Uighurs to Manichaeism while they were still on the steppe but seems to be totally un-

aware of the earlier role of the Sogdians in the empire of the Turks.

One could go on but space will not permit. There is certainly room for a new interpretive outline of the role of Central Asia in world history, from the first harnessing of the horse to the war chariot until the eastward expansion of the Russians, but this book, regrettably, does not fill that need.

EDWIN G. PULLEYBLANK
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ROBERT JAY LIFTON *et al.* *Six Lives, Six Deaths: Portraits from Modern Japan*. New Haven: Yale University Press. 1979. Pp. xiii, 305. \$16.95.

In this collaborative effort a prominent American psychohistorian, Robert Jay Lifton, and a Japanese man-of-letters, Shūichi Katō, assisted by a graduate student, Michael R. Reich, study six "deaths and lives as a way of exploring issues of history and social changes" (p. 289). In the first chapter Lifton explains that this psychohistorical study incorporates Erik Erikson's perspective on the "great man or woman in history" and his own shared-themes approach—namely, "a psychological investigation of a group of individuals who commonly experience an important historical event" (p. 5). In this instance the authors focus on six separate lives and deaths in terms of their special significance and examine the intermeshing of three levels of collective historical, cultural, and universal experience in their subjects' lives. Death and continuity is the paradigm used, and the theme of symbolic immortality, Lifton's hobbyhorse, remains constantly in the background.

In linking the lives and deaths of individuals to the larger sociohistorical context, the authors assert that revolutionary changes are like historical deaths. In modern Japan two such historical deaths occurred: the Meiji Restoration and the defeat in World War II. These events and the lives of the six men, the authors argue, were closely interlinked. This thesis is borne out especially convincingly in two instances: that of the Meiji Restoration and the "hero" of the Russo-Japanese War, General Nogi, and that of World War II and the novelist, Mishima Yukio. These two men were torn between the traditional and the newly emerging values as their country underwent dramatic changes; both committed suicide by the sword. The others (the novelist-army medical officer, Mori Ōgai; the progressive political theorist-social critic, Nakae Chōmin; the Marxist academic, Kawakami Hajime; and the novelist-literary critic, Masamune Hakuchō) were also riven by the changing times, thinking about and facing death in their own unique and courageous manner; but they did not make as striking an imprint on the Japanese mind as did Nogi and

Mishima. All of these men were alienated outsiders whose (dare I use the word?) neuroses were kindled initially by a demanding or possessive father, mother, or grandmother and fanned by the changing sociocultural environment. Their psychological tensions and obsession with death made them acutely sensitive observers of their society and time and generated in them a creative vitality. Despite the authors' emphasis on death, what is remarkable about these men is the way they lived—with courage, integrity, and moral sensitivity.

This study overflows with incisive, thought-provoking observations, which, I regret, space does not permit me to illustrate. Let one example suffice: "Nogi's particular human combination of achievement, confusion, folly, glory, anachronism, rigidity, despair and determined self-completion . . . was that of the Meiji era" (p. 62).

One may not be fully satisfied that the themes outlined in the first chapter are convincingly delineated in all cases; one may question some of the assertions (for example, the claim that the Japanese made the leap to Christianity because of its powerful image of death and revitalization is contradicted by the authors' own discussion of Kawakami's dramatic leap of faith triggered by his encounter with the Sermon on the Mount), the overloading of some of these men with a medley of psychological hang-ups, and the appropriateness of some of the comparisons with Western personalities (for example, Mori Ōgai with Montaigne), but unquestionably this work is a major contribution to Japanese studies, to the art of biography, and to the discipline of psychohistory.

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CHONG-SIK LEE. *The Korean Workers' Party: A Short History*. (Histories of Ruling Communist Parties.) Stanford, Calif.: Hoover Institution Press. 1978. Pp. xiv, 167. \$5.95.

The first in a series of monographs on the histories of the sixteen ruling Communist parties, this work presents a succinct and readable summary of the origins, structure, and functioning of the Korean Workers' Party (KWP), the ruling party of the Democratic People's Republic of Korea (North Korea). Chong-Sik Lee brings to his task an impressive array of credentials: more than a decade of laborious and path-breaking scholarship on the various aspects of Korean communism and an enviable linguistic competence that enables him to use original sources in the Korean, Japanese, Chinese, and Russian languages.

Indeed, it is precisely because the author is a pioneer in his field that a preliminary question arises:

What, if anything, does he have to add to his already voluminous publications, not to mention to the literature of Korean communism? The question becomes germane, because the author, in collaboration with Robert A. Scalapino, published the award-winning *Communism in Korea* (2 vols., 1972), which, together with Dae-Sook Suh's *The Korean Communist Movement, 1918-1948* (1967), virtually exhausts the topic. The author's own answer is that there is room in the literature for "a shorter work of interpretative nature" (p. xiii). Additionally, he provides a new interpretive analysis of the developments in North Korea since the Fifth Congress of the KWP in November 1970. In short, the present monograph is not merely a summary of the massive Scalapino-Lee study but also contains new interpretations.

To the uninitiated, the following features of Korean communism that emerge from this study may be of interest: First, in its formative stage, the early 1920s, the Korean Communist movement was an offshoot of the Korean independence movement. Second, the Korean Communist movement was plagued by factionalism; the situation was so bad that there were even bloody clashes between contending factions, and the Comintern eventually ordered the dissolution of the Korean party. Third, although riven by factionalism, the movement was remarkably free from theoretical disputes, which Lee attributes primarily to the domination of the movement by the relatively uneducated—a situation that contrasts sharply with the experiences of China and Japan. Fourth, Korean Communists were scattered widely both inside and outside of Korea, notably in China and Russia. Fifth, the movement was singularly unsuccessful. Lee cites as the principal reasons for the Communist failure (1) the brutal efficiency of the Japanese police, (2) Korea's then precapitalist stage, and (3) the adverse impact of the Comintern, which prescribed radical and counterproductive guidelines in disregard of the problems on the scene. Sixth, the *coup de grâce* was administered by Kim Il-sŏng, a man totally alien to the Korean Communist movement, although he did play a part in the anti-Japanese guerrilla operations under the direction of the Chinese Communist Party. Kim all but decimated the remnants of the Korean Communist movement after taking over the reins of power in North Korea under the auspices of the Soviet occupation authorities. Seventh, since 1949 when the KWP was formed, Kim has not only consolidated his power beyond challenge but also transformed the party into an intensely personal one—a veritable tool in the propagation of a cult centering about himself and his legendary family. Finally, the KWP has been grappling with the problem of striking a balance between redness (loyalty) and expertise. As the grow-

ing requirements of industrialization dictate the primacy of expertise, the KWP is becoming increasingly dominated by technocrats.

Many problems remain in North Korea, not the least of which is one of the institutionalization of its political system, for the supreme leader is bound to pass from the scene sooner or later. In sum, this is a useful short book, which is likely to inform and stimulate the layman and the expert alike.

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SUKHDEV SINGH CHARAK. *History and Culture of Himalayan States*. Volume 1, *Himachal Pradesh*, Part One. Foreword by S. C. DUBE. New Delhi: Light and Life; distributed by Humanities Press, Atlantic Highlands, N.J. 1978. Pp. x, 413. \$32.50.

The Himalayan region of the Indian subcontinent contains a diverse range of peoples and cultures whose history, like the terrain they inhabit, consists of isolated fragments. Historical studies of these peoples—as of most others in South Asia—that take indigenous perspectives seriously are only now beginning to emerge. In a massive series, projected to extend to ten volumes, Sukhdev Singh Charak attempts to provide a comprehensive account of the social, cultural, and political history of each of the states of the entire Himalayan region. The first volume, considered here, treats Kangra and related states in present-day Himachal Pradesh.

Much of the earlier work on this northerly fringe of South Asia consists of studies by the British imperial government, which attempted to categorize the people it ruled. From the end of the eighteenth century until independence in 1947, imperial administrators produced gazetteers, settlement reports, and other accounts that classified not only the land but also the social and religious practices of every "caste" and tribe under their authority. Rich in detail and description as such surveys are, they necessarily represent an exogenous vision, often saying as much about the categories of the author as those of the subjects. Much current work seeks to remedy these deficiencies by examining South Asian society as it sees itself.

Although Charak displays an awareness of some of the indigenous sources available to historians of the region, including family histories, epigraphic and numismatic materials, and folk traditions, his decision to deal with such a vast topic apparently forces him to rely primarily on the aforementioned official and exogenous sources. Charak promises to deal with not only the approximately eighty states of Himachal Pradesh but also Assam, the North-eastern Hill States, Jammu and Kashmir, Sikkim,

Bhutan, and "possibly Nepal" as well. To present a "comprehensive study," Charak thus draws almost exclusively on English printed secondary sources. Whole sections of his history are taken directly from other texts, including one fifteen-page section (pp. 30-44) from John Hutchison and Jean Vogel's two-volume *History of the Panjab Hill States* (1933). Later, an exact quotation from another source extends unbroken for ten pages (pp. 283-93). Charak further detracts from the value of his contribution with incomplete footnotes and the absence of any bibliography. The layman may be confused by a mass of occasionally inaccurate dates and inconsistently spelled names: the river Sutlej, for example, also appears randomly as Satluj and Satlej.

Despite these flaws, however, Charak's work draws together a series of separate and less accessible accounts and histories. He takes into account the Himalayan environment that helped fashion these numerous states. He presents us with a picture of each as it struggled, apparently perpetually, for its political autonomy and cultural identity against outsiders: other hill states and the larger Hindu, Muslim, and British empires of the plains. We can further hope that Charak's ambitious effort will serve to stimulate interest in the region, providing landmarks for future research.

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J. VAN GOOR. *Jan Kompenie as Schoolmaster: Dutch Education in Ceylon, 1690-1795*. (Historische Studies, number 34.) Groningen: Wolters-Noordhoff. 1978. Pp. 205. f 35.

Nineteenth-century European imperialists in Asia controlled wide territories, systematized (or transformed) traditional taxation systems in search of revenue and stability, created a dependent local elite and attempted to restrain its exploitation of the peasantry, and saw in the spread of missions and of European-style schooling sources of political strength for the empire as well as self-sufficient religious and cultural values. J. Van Goor's groundbreaking book allows us to see Dutch policy in Ceylon (now Sri Lanka) in the eighteenth century as one of the most important and revealing precursors of all of these nineteenth-century phenomena. The author's choice of the schools as the focus for his study was an excellent one. Indigenous schoolmasters, apparently usually members of prominent local families, kept the records for Dutch exaction of local taxes and labor services. The Dutch authorities saw the conversion to Protestantism of their Roman Catholic, Buddhist, and Hindu subjects, largely through these local schools, as one of the most effective means of assuring their loyalty. The

seminaries in Colombo and Jaffna were to produce the necessary schoolmasters, indigenous ministers, and so on for this missionary effort and also were to provide European-style education for sons of Dutch residents and for sons of the indigenous elite who sought to qualify for full-time employment by the Dutch East India Company or for rural posts that reinforced customary dominance.

Van Goor's study is based on wide research in the great archives of the Dutch East India Company, Dutch church archives, and a small amount of material in Sri Lanka. It has required an extremely conscientious piecing together of bits of information (1,097 footnotes for 144 pages of text!) and is full of fascinating detail on the differing responses of castes and ethnic groups to the schools. Appendixes list the names and careers of individuals known to have attended the seminaries. We see the school system staggering under its multiple functions, always starved for funds and personnel by the cost-conscious company, facing cultural obstacles that few Dutchmen seem ever to have understood. Permanent and genuine conversions to Protestantism were scarce except in Colombo and its environs, and the seminaries were more effective in producing a bicultural and multilingual indigenous elite than in training mission personnel.

In view of the arduous research and very substantial thought embodied in this book, it may seem unfair to say that it would have been much better if the author had taken a year or two to expand and revise it into a more polished piece of work with more substantial treatment of background and of some of the issues of interpretation implicit in it. The Dutch system of quick publication of the doctoral dissertation, admirable in many ways, simply is not adequate for a piece of research that opens up as much new territory as this one does. As it stands, the book is sprinkled with *obiter dicta* and with inadequate summaries of policies and events. It needs coherent background expositions of the author's understanding of the nature of Sinhalese society and culture and of the main stages of Dutch policy in politics and commerce as well as in education. Many aspects of the Dutch effort can be very fruitfully compared with Roman Catholic missionary and educational policies in Asia, but Van Goor has done very little in that line. This is a premature publication of exciting research that will be of interest to every student of European empire in any part of maritime Asia.

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WESTON BATE. *Lucky City: The First Generation at Ballarat, 1851-1901*. Carlton, Victoria: Melbourne Uni-

versity Press; distributed by ISBS. Forest Grove, Oreg. 1978. Pp. xv, 302. \$20.00.

Few permanent cities grew out of the gold fields that so captured the minds and imaginations of the nineteenth century. But one that did endure was Australia's Ballarat; and, although it is no Johannesburg or Denver, it is a sturdy provincial city with an individuality and tradition all its own. That Ballarat did not become a ghost town, as so many other gold rush sites did, was due to a variety of factors: the continuing richness of the gold yield, the town's proximity to a rich and developing agricultural region, and a willingness on the part of those made wealthy by the mines to invest in local industrial production and later to enrich the community life by their philanthropy. Both Anthony Trollope and Mark Twain found much that was agreeable during their brief visits to the town.

Weston Bate has told the story of the first generation in Ballarat, a rather long-lived generation by some standards, since he carries the account down to the turn of the century. His work is essentially a municipal biography; the overwhelming bulk of the author's sources are literary, with a heavy reliance on the Ballarat press of the day. Adherents of the newer approaches to urban history will find the work old-fashioned. And, for all the fullness of the text, there are some odd omissions. Despite the dependence of the town on the local mines for many decades, there is little discussion of changes in mining technology; metallurgy is not even listed as a topic in the index. Nor does the author seem concerned about the health problems related to mining, although public sanitation is the central matter in his discussion of the general health of the community.

Despite such deficiencies and the rather graceless literary style, *Lucky City* is a commendable addition to the growing number of works on Australian urban history. Melbourne University Press has made a handsome book of it, with a lavish number of illustrations.

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UNITED STATES

JOHN DEMOS and SARANE SPENCE BOOCOCK, editors. *Turning Points: Historical and Sociological Essays on the Family*. (*American Journal of Sociology*, volume 84, supplement 1978.) Chicago: University of Chicago Press. 1978. Pp. xi, 413. Cloth \$20.00, paper \$10.00.

Several years ago, Daniel Scott Smith proposed that "a systematic history of the American family can be

reconstructed if sociological theory, long-run series of quantitative data, and historical imagination in devising subtle measures of change are combined." *Turning Points* advances research in family history in the direction Smith suggested. Conceived in 1975 by the Russell Sage Foundation Program on Age, the book benefited from extensive exchanges among its authors and editors (a historian and a sociologist) as well as outside review before its publication as a supplement to the *American Journal of Sociology*. The collection of essays is a valuable contribution, not simply for its theoretical insights and substantive findings, but for its demonstration of the fruitful ways that interdisciplinary collaboration can improve historical scholarship as it enhances non-specialists' familiarity with historians' materials and methods.

Turning Points has four parts. In the first section, Glen H. Elder, Jr., offers a critical appraisal of pivotal sociological inquiries into the family and identifies conceptual and methodological issues confronting scholars who investigate family and social change over time. The historians' essays in the second section emphasize transitions to and from the family. Carl Kaestle and Maris Vinovskis, employing nineteenth-century Massachusetts census data and school records, analyze shifts in the manner families and society dealt with children from ages three to six. Michael Katz and Ian Davey's comments on the cultural-historical origins of adolescence, parent-child relations, and the social consequences of public education rest on evidence from a larger case study of Hamilton, Ontario, between 1851 and 1891. Adopting a life-course approach, John Modell, Frank Furstenberg, and Douglas Strong persuasively argue that the timing of first marriages, and its relation to other passages to adulthood, did not change significantly from the late 1800s until 1940. Tamara Hareven provides the book's most provocative and impressive essay, an examination of the dynamic and multi-faceted role of kinship among French-Canadian immigrant textile workers in Manchester, New Hampshire, from 1880 to 1936.

Attention in the third section turns from studies of overt behavior to explorations of certain norms defining or prescribing appropriate age and sex roles in the past. Joseph F. Kett traces the connotations of "precocity," focusing on the ramifications of commentators' animosity to adult behavior in adolescents between 1830 and 1930. Carroll Smith-Rosenberg offers an intriguing entho-historical analysis of Jacksonian America as she relates Victorian sexual repressions, phobias, and fantasies to broader conditions in society and the family. In the most compelling analysis of old age in early America currently available, John Demos contends that the position of the elderly in colonial New Eng-

land was sociologically favorable but psychologically disadvantageous.

The sociologists' commentaries in the last section should be read by all social historians. The essays by Rosabeth Moss Kanter and by Neil Smelser and Sydney Halpern place the interrelationships among individual life histories, family processes, and structural changes in education and the economy into broader theoretical perspective. Anne Foner applies an age stratification model to make stunning inferences about age relations in nineteenth- and twentieth-century families, thereby elucidating key changes in family patterns. Sarane Spence Boocock presents a judicious critique of the strengths and weaknesses in the seven historical essays and useful suggestions for alternative research designs in future studies.

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BARBARA J. HARRIS. *Beyond Her Sphere: Women and the Professions in American History*. (Contributions in Women's Studies, number 4.) Westport, Conn.: Greenwood Press. 1978. Pp. x, 212. \$15.95.

The core of the argument in Barbara J. Harris's new book is that traditional assumptions about woman's intellectual inferiority and domestic obligations have restricted her career opportunities. There is nothing especially novel about this theme and the book is frankly intended to serve as an introduction to the field. It does provide an intelligent review of the secondary literature on this ideology, but it does not go beyond what the informed reader will already know.

Harris first traces the development of the "cult of inferiority" from its origins in pre-modern Europe through its transplantation to the American colonies. Moving through four hundred years of history in twelve pages she must be highly general and so we read that "traditional misogynous ideas were thus part of the ideological inheritance of the new nation and constituted one of the major obstacles to females who openly expressed professional aspirations for themselves or their sex early in the nineteenth century" (p. 22). The "cult of domesticity" in the nineteenth century built upon these earlier notions and again restricted female career advancement in England and America.

Believing that these ideologies effectively deterred most women from leaving home, Harris focuses her next chapters on the few American women who rebelled, those who established female colleges (to challenge the notions of the cult of inferiority) or who entered professions such as medicine or law (to challenge the assumptions of the cult of domesticity). But despite these efforts the ideology persisted,

at least through the 1960s. Harris's two concluding chapters treat post-World War II America, particularly the increase of women workers and the growth of a new feminist ideology. Curiously, she makes few links back to her own material. Although her book provides some analysis of the current situation of professional women, she does not explore why so persistent an ideology finally lapsed.

Harris wants her historical analysis to prod women today to action, to encourage them to continue the fight begun by their "noble predecessors" (p. 191). But this approach is really of little help, either to history or to social policy. By focusing on the ability of a few women to overcome barriers, Harris becomes a woman's counterpart to the efforts of immigrant historians to glorify the achievements of a few newcomers. These historians also celebrated those who made it—and by implication suggested that those who failed had themselves to blame. In this same spirit, Harris's glorification of the rebellious women limits the possibilities for a deeper analysis, a more structural approach to the barriers that professional women confronted. She excluded lower-class women from her book as "peripheral to my subject" (p. x); ostensibly, they would be more affected by external circumstances. Perhaps in that way Harris thought she could avoid analyzing external, social considerations—as though middle-class women were in control of their fate. But such an argument will not do, and the history of the rebellious few makes this altogether apparent.

First, the earlier leaders were not nearly as rebellious as Harris would have it. She relates the establishment of a profession such as nursing to the work of a handful of courageous women to expand opportunities for their sex. But the development of this profession (like the expansion of teaching and the rise of office work, both of which occurred at the same time) demonstrates the power of stereotypic notions to keep women in their proper place even when they left the home. Harris cannot take note of the dynamics that led to the sex-stereotyping of woman's work. Her analysis neglects to consider that positions which became women's positions were the ones than men with the same training would not assume. In effect, she gives her rebellious few too much credit and underestimates the dead-end character of the posts that women occupied, even when they were outside the factory. Not surprisingly, the rebellious few who did manage to enter a male profession had little impact on the norms of the profession. Harris's volume contains statistics demonstrating how women now are crowded into a few jobs and how they suffer from unequal pay. But none of this prompts Harris to consider underlying causes, to ponder whether heroic types are sufficient to the task of promoting social change.

In the end, it is not enough to encourage women

to "take heart from the experiences of the heroic women who opened higher education, law . . . and countless other opportunities to members of their sex. . . . The debt should be repaid by continuing the crusade to build a society founded on sexual equality." We need, in addition, an understanding of the dynamics that operate within social institutions so that the expansion of opportunity for some will become synonymous with the equality of opportunity for all.

SHEILA M. ROTHMAN
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H. ARNOLD BARTON. *The Search for Ancestors: A Swedish-American Family Saga*. Carbondale: Southern Illinois University Press. 1979. Pp. xi, 178. \$11.95.

Historians who view genealogy with condescension would do well to examine this model combination of history and genealogy written by H. Arnold Barton, the editor of a volume of immigrant letters, *Letters from the Promised Land: Swedes in America, 1840-1914* (1975) and editor of the *Swedish Pioneer Historical Quarterly*. With exhaustive research in Sweden and America, including extensive interviewing, and with a background in Scandinavian history, the author gracefully traces his family back into the sixteenth century, placing each generation in the context of the historical developments of its time through to the time of emigration in mid-nineteenth century and thence to the many settling places on the American continent.

The place of the author's family origins may be familiar to those who have read the novels of Vilhelm Moberg, who came from those same rocky uplands of southern Småland in Sweden. Although not all of the author's ancestors came from this area, the main lines seem to have stemmed from the parishes of Södra Vi, Djursdala, and Odensvi. One line is traced back to 1538, but the more useful records are from the eighteenth century forward. One gets an impression of a good deal of movement of persons from parish to parish and from one class or occupation to another, together with much cross-fertilization of stock. As shown later in the volume, not until the third generation and after did Swedes intermarry with persons of other groups to any extent. The circumstances of the growing emigration after 1840 are clearly indicated.

The familiar story of the Atlantic crossing is sketched for the author's families, but the major part of the latter half of the volume deals with the process of dispersal and acculturation in the United States. The varying degrees of acculturation are brought out with some indication of survival of interest in ethnicity. In an appendix on "Sources and Problems" the author explains his methodology and

the special problems involved in this type of research. Anyone entering this field, regardless of ethnic group, would benefit from this essay. There is detailed documentation for each chapter, a good index, and some illustrations. One or two maps would have been helpful.

CARLTON C. QAULEY
Minnesota Historical Society

DAVID MALDWIN ELLIS. *New York: State and City*. Ithaca: Cornell University Press. 1979. Pp. xi, 256. \$9.95.

For those who desire a brief treatment of New York State that encapsulates the entire timespan of the region's development, this book will fulfill many expectations. David Maldwyn Ellis's objectives are mainly to elucidate significant themes. Written in rapidly moving, almost newsmagazine-style prose, this survey devotes perhaps one-third of its space to the pre-1825 period and the balance to the subsequent century and a half. The author strives to interconnect politics, economics, and culture and is most successful in the three chronological chapters to 1825. There are separate chapters on the state's ethnic groups, character, economy, culture, political parties from 1825 to 1977, and the upstate-downstate rift.

A unique combination of abundant natural resources, geographic location, and the character of the multiethnic population have affected the state's most notable achievements. An abundance of land in the preindustrial period lured large numbers of migrants whose productivity sustained the drive for commercial leadership by New York City's merchants. Later, industrialization and urbanization afforded jobs for those who forsook their roots for New York. Integral to this process, triggered and bolstered by revolutionary changes in transportation technology, was the state's access to our west and the world. Although old Yorkers and transplanted Yankees predominated numerically, morally, and socially prior to the Civil War, subsequent population migrations restructured the demographic base, imparting new force and direction to socioeconomic and political alterations. These alien residents won ultimate acceptance and nourished the improvement of the general welfare.

Ethnic mix and an ability to discover means "to accommodate change" (p. 24) rank among the important characteristics of a citizenry whose headlong pursuit of materialism had its negative aspects. Still, Ellis awards the state high marks for a receptivity to innovation in several spheres that offsets "crassness" and occasional "tribalism."

Another theme, the upstate-downstate discord, is an ineluctable topic. The author duly touches major historical issues (for example, home rule, control

of the legislature, and distribution of state fiscal revenues), but his explanation of the hostility is insufficient because the problem's roots have permeated almost all facets of life. Ellis attributes the causes of the phenomenon to racial, religious, cultural, and political antagonisms, all of which indeed have played a role, but, for example, these elements have changed over the last century, a broad range of economic and social interest groups have manipulated them, fearful perceptions of a reordering of family and sexual relationships have taken their toll, and the abysmal failure of our institutions to inculcate civility and a decent respect for the humanity of others have exacerbated them. Amid the stresses of industrial and urban life, New York City deviated sharply from upstate-conceived conformity and so became the sacrificial lamb, despite Ellis's contention that the urbanites had their own contemptuous attitudes toward the upstate "benighted yokels" (p. 198). This poisonous situation, however, is not a matter for the impartial distribution of responsibility between both regions; rather, it signifies a depressing, long-term bankruptcy of the state's leadership.

One of the problems with "panoramic overviews" is that some readers have a different conception of the panorama. Undergirding familiar notions of industrialization, urbanization, and ethnicity, among others, the author has assumed the desirability of growth and society's ability to cope equitably with its strains. Nevertheless, the book is murky on the nature of preindustrial society, on the process, costs, and profound social impact of industrialization or "modernization." Moreover, the scant references to family relationships, feminism, the Great Depression, the politics of the thirties and forties, the infection of McCarthyism, Vietnam, and environmental and nuclear-energy perils are serious deficiencies.

An assessment of Ellis's accomplishment must be two-fold. This volume will please those who want a descriptive sketch of the more traditional thoroughfares of this state's history. For those seeking an explanatory narrative that attempts to account for the state's growth along the lines indicated above, *New York: State and City* will be disappointing.

BERNARD MASON

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THEDA PERDUE. *Slavery and the Evolution of Cherokee Society, 1540-1866*. Knoxville: University of Tennessee Press. 1979. Pp. xiv, 207. \$12.50.

Cherokee cultural adaptation to the dominant white society and the resulting factionalism between traditionalists and progressives has been explored and explained numerous times with varying

interpretations. Economic self-interest, internal political power struggles, and ideology are popular themes for explaining the phenomenon. Theda Perdue contends that slavery played a larger role in this change and struggle than has been supposed and that it "helped shape the economic class structure and conflicting value systems which produced the persistent factionalism" (p. 144). Perdue's thesis is an intriguing one, but at times her approach is frustrating. Frequently, too much space is allotted to surveying the familiar story of Cherokee society without investigating the impact of slavery on the tribe or the internal workings of the slave system.

The Cherokees practiced aboriginal bondage but with great dissimilarities to the system developed by whites. Slavery among the ancient Cherokees was not based on the economic rewards of owning slaves, nor was status attained in owning bondsmen. Indeed, slaves who were not adopted into the kinship system were practically valueless. The adoption of and desire for European manufactured goods changed the nature of Indian slavery. This period of adaptation saw the large and powerful Cherokees make war on lesser tribes in order to gain captives for the slave trade. Here then began a singular change in traditional concepts. Warfare in this new situation was not based on revenge or restitution but on obtaining captives for barter. Slaves now had a definite economic value.

The Cherokees soon discovered that trade in black slaves was much more profitable and less costly than tribal warfare. Tribal members became adept slave-stealers and traders during the eighteenth century. Plantation slavery "developed only after the alteration of . . . the kinship system, the division of labor, and the political system" (p. 50). By the 1820s and after the development of an elaborate legal system, slave codes and restrictions on Negroes became an integral part of Cherokee society. Perdue found no hint of desertion on the part of slaves, much less any insurrection of the blacks, until after removal. She couples the outbreak of such incidents to the disruption of Cherokee society under removal. Perdue explains the increasing severity of the Cherokee slave codes in the West as a surrender to "white" supremacy and a growing racism.

Readers will probably be most interested in the chapter on "Masters and Slaves," hoping to discover there how the slavery system worked among the highly acculturated Cherokees. One especially wants to know about the basic relationships between the owners and their bondsmen. While expressing a belief that the institution was generally a lenient one, the author gets diverted on peripheral issues, and the chapter is weakened and becomes somewhat disappointing. Why, for instance, was valuable space assigned to scanning Cherokee wealth in nonagricultural pursuits, such as steam-

boating, without linking such activities to slavery? Perhaps the sources are not adequate to answer the more difficult questions on the red man's relations with his black bondsmen. Outside of the WPA interviews of the 1930s in the "Indian-Pioneer History," no extensive body of material exists. Perdue, however, did make full use of the available resources and has compiled an impressive bibliography.

Although the first chapters are stronger than the latter ones, the work as a whole exceeds any previous study in this area. The author may certainly be complimented on her scholarship and writing style. Given the nature of the topic and the book's merits, the work should find a place on any library shelf and in private collections as well.

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PATRICIA K. OURADA. *The Menominee Indians: A History*. (Civilization of the American Indian Series, number 146.) Norman: University of Oklahoma Press. 1979. Pp. xx, 274. \$14.95.

Peacefully inclined Indian groups complained in the nineteenth century that the United States government neglected them in favor of more warlike tribes. Somewhat the same charge can be leveled against historians: warrior societies, especially those from the Great Plains, have received more attention than the relatively inconspicuous tribes. Happily, in recent years historians have been redressing the balance and giving attention to previously neglected groups.

The Menominees, though willing and able to defend themselves, never fought a major war against the United States. Miraculously, they were allowed to retain a portion of their aboriginal territory as their reservation. So successfully did they appear to be coping with the problems of acculturation that they were among the first tribes chosen for the termination of government services in the 1950s. The disastrous aftermath of termination—and the subsequent restoration of their former status—brought them to public attention and presumably led to the writing of this full-scale tribal history.

Patricia K. Ourada's history of the Menominees departs from the usual practice of ending somewhere about 1900 and carries their story through 1975, when the tribe's relationship to the government was restored. In fact, more than a fifth of the book deals with the twentieth century. The tribe's earlier history is not ignored, however. After a rather thin survey of Menominee culture, Ourada traces their successive relations with the French, the

British, and the Americans and details the gradual reduction of Menominee territory and the pressures for acculturation in the nineteenth century. In a chapter on the early twentieth century she attempts to show how the tribe progressed economically to a point where some of its own members and many outsiders believed that it could get along without further protection and supervision by the Indian Bureau.

There is a limit to what a book can accomplish in less than 225 pages of text, however, and the reader may have a sense of being hurried from one historical period to the next, without acquiring any real familiarity with any of them. Plenty of facts are presented, but rarely does the author explain their significance or their relationship to one another. The result is a certain choppyness, less evident in the chapters dealing with recent events than elsewhere.

The book is also marred by carelessness of diction—as when "extract" is used for "exact" (p. 37) and "forestalling" for "postponing" (p. 209). The index is adequate but by no means complete; for example, no entries are provided for Stockbridge, Munsee, or Oneida, three tribal groups whose reservations were carved out of Menominee land. The two maps are too small to be of much value, and many geographic names mentioned in the text do not appear on them.

Despite these faults, *The Menominee Indians* is a worthy addition to the University of Oklahoma's Civilization of the American Indian Series. It makes a unique contribution as the case history of a tribe that underwent termination—and was able to reverse what nearly everyone assumed was the inevitable course of history.

ROY W. MEYER
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GEORGE PIERRE CASTILE. *North American Indians: An Introduction to the Chichimeca*. New York: McGraw-Hill. 1979. Pp. xiii, 314. \$12.95.

Do not be misled by the title of this book. Although the word "Chichimeca" is usually applied only to the tribes on Mexico's northern border, George Pierre Castile uses the term to refer to virtually all of the Indians of North America. The book is designed as a college text for courses on the Indian in America. Although such courses were formerly based on anthropology alone and fixed in the "ethnographic present," they are increasingly rooted in a historical framework. Yet they have remained in anthropology departments and have been taught by anthropologists. The present book, which is organized in a chronological manner and divided into pre-European contact and postcontact halves, is more historically based than previous essays in In-

dian history by anthropologists. At the same time each chapter is keyed to a specific cultural topic.

Castile writes in a breezy style, peppering his book with catchy section headings. He thus sugar-coats some of the heavy anthropological jargon that is *de rigueur* among anthropologists. But the sugar coating is not bastardization or oversimplification. It is good and effective popularization. Other virtues of the book are Castile's healthy skepticism concerning some of the "metaphysical wrangles of anthropological theory" (p. 61), his use of pungent analogies from non-Indian cultures (for example, European feudal relationships to help explain Indian kin, rank, and class relationships), his refusal to take the fashionable ideological bias of glorifying contemporary Indian radicals and condemning elected tribal leaders, and his refreshing modesty in admitting his inability to determine the answer to "the Indian problem."

In sum, this is a refreshing, erudite, and witty account of the American Indian from twenty thousand years ago to the present. While anthropologists have a lot to learn from historians, historians probably have more to learn from anthropologists in this as in other fields of history into which anthropologists are moving. If readers are not misled by the title, this book should find a solid place in college curricula dealing with the American Indian.

WILCOMB E. WASHBURN
Smithsonian Institution

R. DAVID EDMUNDS. *The Potawatomis: Keepers of the Fire*. (Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1978. Pp. xii, 367. \$19.95.

The University of Oklahoma Press Civilization of the American Indian Series has, as one of its most recent volumes, this excellent tribal history of the Potawatomis. R. David Edmunds has given us a comprehensive, heavily annotated account of these people and their white contacts from the seventeenth century to the era of removal in the 1830s. The story of the Potawatomis is complex, involving a number of separate bands, some of them difficult to identify as Potawatomis. Indeed, as Edmunds points out, the Potawatomis and the Chippewas were believed to have been one tribe by early white observers in the seventeenth century. And throughout the whole colonial period the Potawatomis were often close allies and neighbors of other Algonquian woodland peoples, including the Ottawas, the Winnebagos, the Miamis, the Sacs, the Foxes, the Kickapoos, and others. In the flood of detailed narrative, however, Edmunds identifies bands that were clearly Potawatomis, those of Detroit and St. Jo-

seph, those living along the Wabash and Illinois Rivers, others who lived among the Hurons, and those called the Prairie and Woods Potawatomis.

These people, as well as their Algonquian neighbors, were excellent farmers whose fields were planted with corn, beans, squash, melons, pumpkins, and tobacco. Potawatomi women harvested a bonanza of wilderness crops, particularly wild rice, roots, berries, and maple sugar. Surplus food was stored in clay pots, skin bags, and baskets. And this rich diet was supplemented by fish and meat. Potawatomi woodland craftsmen made superb dugout and birchbark canoes, arched-roof bark houses for summer, and wigwams for winter.

This Potawatomi woodland lifestyle was, of course, gradually altered with the coming of the whites. Edmunds continually stresses the trade motive in his history of Potawatomi-white relations, which began with exchange of surplus corn with the French for weapons, tools, and liquor. Eventually the Potawatomis, whose history is closely identified with those major tribes of the Great Lakes, were dispossessed, their chiefs bribed with expensive gifts to sign away a landed heritage. There was a familiar pattern of Indian resistance, defeat, and an impact of disease usually coming along with the white man's missionaries and Indian agents.

In the early years of white contact the Potawatomis established friendly relations with the French. Some of them were converted by Jesuit missionaries. They traded with LaSalle and fought with Frontenac against the Senecas. According to the author they were a key factor in the French victory over Braddock. Later they aided Pontiac in his rebellion, and they were among the Indians who warred against William Henry Harrison. At the Treaty of Fort Wayne, the Potawatomis and their Indian allies signed away some three million acres. Here, then, is a story of war, treaties, defeat, and removal. Edmunds's complex narrative is essentially the history of a tribe that was gradually expelled from a vast territory in the Northeast and upper Middle West.

The full force of the American frontier advance was not to be denied. Not without justification the Potawatomis complained that "the plowshare is driven through our tents." As they came under the control of the American government in the nineteenth century, they were forced to submit to the will of their conquerors. Though they valued blacksmiths rather than missionaries and preferred Catholic rather than Baptist missionaries, their wishes were usually overlooked. Their forced removal westward to Kansas was, as Edmunds states, "plagued by hardship."

Edmunds's summary commentaries state that the removal with its hardship was also marked by "fraud and chaotic planning" and that removal

treaties "were fraught with criminality." Certainly, the University of Oklahoma Press is to be congratulated in giving us an authoritative account of this chilling part of our national history. Edmunds's thorough research and his evocative book are truly significant contributions to American Indian history and Indian-white relations.

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BRUCE G. TRIGGER, editor. *Handbook of North American Indians*. Volume 15, *Northeast*. Washington: Smithsonian Institution. 1978. Pp. xvi, 924. \$14.50.

This volume, the second to be published of the twenty-volume *Handbook of North American Indians* (General Editor, William C. Sturtevant), covers the history, cultural background, and present circumstances of the Indian peoples of the northeastern United States and southeastern Canada. Supported by federal appropriations to the Smithsonian Institution, partly through its Bicentennial Programs, the series is intended to supersede F. W. Hodge, ed., *Handbook of American Indians North of Mexico* (2 vols., 1907-10), which has been a standard reference work for more than half a century.

The new *Handbook* differs significantly from the original one. It not only has greatly expanded and updated Hodge's *Handbook* by fully incorporating the results of the research done during the past four or five decades but also has adopted a new approach. The seventy-three refereed articles, written by forty-seven anthropologists, five historians, one sociologist, and one geographer, are arranged not alphabetically but topically in four major categories: general prehistory, the Coastal region, the Saint Lawrence Lowlands, and the Great Lakes-Riverine area.

The volume has succeeded in accomplishing two apparently contradictory objectives. While the work is basically a reference encyclopedia, providing a well-balanced summary of each topic suitable for general readers, it nevertheless maintains high professional standards of theory, method, and accuracy. The essays read as a series of scholarly monographs. There is a remarkable evenness in the quality of so diverse articles, and Bruce G. Trigger's introduction and concluding chapter, which categorizes the Northeastern Indian cultures into six major patterns, serve to unify the volume.

The *Handbook* consists of both topical and tribal essays. Among the noteworthy topical essays are Brasser's vivid discussion of early Indian-European contacts, Washburn's concise and crisp chapter on the seventeenth-century Indian wars, Fenton's illu-

minating "Northern Iroquoian Culture Patterns," Trigger's comprehensive article on early Iroquoian contacts with Europeans, Tooker's perceptive essay on the League of the Iroquois, and Wallace's "Origins of the Longhouse Religion," an excellent synthesis of his earlier works.

The tribal chapters provide detailed information on each of the Indian groups. Repeatedly, the complexity of Indian history is recounted: major geographical dislocations and the formation of larger tribal units in response to early European contacts, their disintegration as a result of growing white pressures, and the recombination of refugees to form new, heterogeneous, and often resilient groups. These chapters are frequently repetitive, due to many common characteristics among all the Northeastern tribes, such as division of labor, dual chieftainship, customs regarding birth, child-rearing, funerals, mourning, and menstruation seclusion and other sexual practices. The tribes within one region also shared the same cultural pattern. The contributors, however, have made conscientious efforts to single out the unique character of each tribe. The tribal essays include Jennings's brief but penetrating "Susquehannock," Heidenreich's rich, authoritative article on the Huron, Sturtevant's short but revealing essay on the Oklahoma Seneca-Cayuga, Lurie's balanced history of the Winnebago, and Clifton's solid "Potawatomi."

The format follows the style of the *American Anthropologist*. References appear in the body of the text with a unified bibliography at the end of the volume. "Synonymy" in each tribal chapter and a brief bibliographical essay at the end of each chapter are highly useful. Illustrations include numerous photographs, maps, drawings, statistical tables, and charts hitherto not printed. The encyclopedic value of the volume could have been enhanced if the index, though extensive, were more thorough; items like crimes, dreams, intermarriage, and interpreters should have had separate entries, while some existing entries like adoption and gift exchange could have been expanded.

In sum, this is a superb work that will become a standard reference book for many decades. The volume, having a strong anthropological approach, should be particularly valuable to historians in broadening their interdisciplinary perspective. The articles, written in full sympathy with and understanding of the Indian cultures, will also have significant effects on living Indian people and their relations with the larger society. If the rest of the series, scheduled to be out soon, lives up to the same standard, the *Handbook* will indeed be a major and most impressive achievement of our generation.

YASUHIRO KAWASHIMA
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ROBERT KELLEY. *The Cultural Pattern in American Politics: The First Century*. New York: Alfred A. Knopf. 1979. Pp. xiv, 368. \$15.00.

This first installment of a two-volume overview of American political history demands the attention of all political historians. Robert Kelley has audaciously undertaken to synthesize recent scholarship emphasizing ethnocultural determinants of voting behavior with the older scholarship emphasizing economic issues. It is an impressive effort, enriched by the transatlantic comparative perspective developed in Kelley's previous work. He has digested practically everything useful in the enormous literature. Though acknowledging a "tilt toward cultural explanations" (p. 14), he has consistently sought to do justice to alternative interpretations.

The result gives us a more comprehensive appreciation of ethnocultural forces: the central role of Scots-Irish Presbyterians in the coming of the American Revolution in the Middle Colonies; the appeal of the Jacksonian Democratic party for cultural "outsiders" like Irish Catholics in the North, and of the Whig party for cultural "outsiders" like French Cajuns and Appalachian mountaineers in the South.

There are, however, many difficulties. In some crucial cases Kelley's "tilt toward cultural explanations" causes him to push cultural explanations further than I find convincing, as when he sees the American Revolution primarily as a conflict between Anglicizers and the "outgroups" of British-American society—Dissenters, Scots, Irish, and Welsh. In other cases, the argument is bedeviled by internal contradictions. For much of the nineteenth century, Kelley's broadest cultural categories are New England Yankees and Southerners, the former putting their stamp on the Whig and Republican parties and the latter on the Democrats. Yet he has to acknowledge that New Hampshire and Maine were banner Democratic states in the Jacksonian years, while Whiggish "outsiders" divided the South almost equally with the Democrats.

The root problem is theoretical and conceptual. Kelley divides political impulses into economic and cultural, with sometimes one and sometimes the other playing the dominant role. His concept of culture "refers to the realm of consciousness, as distinct from that of the material world and its demands" (p. 10), and thus includes intellectual and ideological factors. So far so good. Yet he waffles on the crucial question of the relationship between the cultural and the material, proclaiming culture to be "independent of (though in interaction with) the material setting" (p. 11), whatever that may mean.

Certainly the interaction between the two remains unexplored. But if consciousness is shaped by

material circumstances (as, among others, Lee Benson has argued in the "Postscript" to his *Turner and Beard* [1960]), the case for an independent cultural factor becomes tenuous indeed. Thus Ronald P. Formisano has pointed out, with reference to Kelley's cultural Southerners, that "from a territorial and economic base Southernness became a cultural difference" (*AHR*, 82 [1977]: 572).

By taking cultural differences as "givens" and failing to explore their linkages with material circumstances, Kelley has finessed the central problem of political historiography. That is why his account, rich and valuable though it is, misses so much that lies at the heart of our history: the material distinctions that made egalitarian and democratic impulses so dynamic a part of Jeffersonian and Jacksonian politics; class situation as a shaper of the cultural consciousness of Irish workers in Jacksonian Philadelphia or Brahmin entrepreneurs in Boston; and in general the different responses of various groups to industrialization and urbanization as mediated through the lens of culture.

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PHILIP J. SCHWARZ. *The Jarring Interests: New York's Boundary Makers, 1664-1776*. Albany: State University of New York Press. 1979. Pp. xvi, 343. \$40.00.

The history of intercolonial boundary disputes in America has been told and retold as part of the larger subject of intercolonial relations within the British Empire. Focusing on New York, several scholars in recent years—Patricia Bonomi, Michael Kammen, Sung Bok Kim—have considered its territorial controversies in their studies on related subjects; now, for the first time, New York's "jarring interests" of this nature appear in full detail in Philip J. Schwarz's book. If "so much tedious detail seems to be involved," to quote from the preface, his admixture of cultural, economic, and social history gives due emphasis to the *dramatis personae* involved in "the politics of interest," and he includes portraits of some who played major roles—Cadwalader Colden, Robert Livingston, Jr., and William Smith, Jr.

New York, bordering on five other colonies, provides the best vantage point from which to narrate and interpret the complex issues of the perennial boundary controversies. New York's status as a continuous royal colony gave it some advantage over chartered and proprietary colonies in its close ties with the Stuarts in the seventeenth century and its traditional loyalty when territorial disputes were taken in hand by the crown. As Schwarz points out, the issues derived basically from local occupation

and ownership of land, contested by physical violence as well as polemics, but extended into the larger arena of territorial jurisdiction and administration. The pressure of increasing population exacerbated the issues, not confined to feuding settlers but rising to the provincial level where territorial claims were of primary concern and thence through colonial agents in London to ultimate royal authority.

The inhabitants in areas of conflict "revealed the power of the powerless," as the author aptly puts it, "by voting with their feet" (p. 98), in the manner of successive generations of frontiersmen. It was, for example, the land riots of 1766 that led eventually to the Hartford Agreement of 1773 on the boundary between New York and Massachusetts. Factionalism, characteristic of colonial New York's politics, with its powerbase in the landed aristocracy, inevitably thrived on intercolonial territorial disputes.

Periodic conflict in New York over its boundaries was a complex development. It is not the stuff that makes for easy reading, but Schwarz has established himself as the authority on the subject. Unfortunately his writing is marred by the current corruption of the language in converting nouns into processions of adjectives—"successful conflict resolution," "frontier defense funding problem," "interest harmonization"—for lack of a well-placed preposition. It is also disturbing to read that the Hudson River "flowed northnortheast rather than due north or northnorthwest" (p. 11), with, one can only conclude, grave historical consequences. Six maps, contemporaneous and later (some of which show the course of the Hudson), complement the text, although necessary photographic reductions have rendered some of them quite illegible.

LESTER J. CAPPON
Newberry Library

PETER N. CARROLL. *The Other Samuel Johnson: A Psychohistory of Early New England*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1978. Pp. 247. \$16.50.

The other Samuel Johnson, a leading eighteenth-century Anglican cleric in Connecticut, is a worthy subject for a book-length biography. Although the colonial Johnson was a distant relative of the English lexicographer, his career was unconnected with that of his more illustrious kinsman. The other Samuel's claims to distinction result entirely from his efforts to expand the Church of England in a colony that was bitterly hostile to Anglicanism. His few successes and many failures as a missionary, the leadership he exercised as first president of King's College, and the reaction of his presbyterially inclined neighbors to his efforts all form a fascinating chapter in early American ecclesiastical history.

The book is noteworthy not only as a biography but as a psychohistory of early New England. The author's definition of psychohistory and his method of attack are best explained in his own words. From the wealth of surviving Johnson material, Peter N. Carroll hopes to "reconstruct an interaction between the man and his culture, . . . observe the formation of a mature personality and explore the subtle relationships between emotional, sometimes unconscious, feelings and more visible behavior" (p. 9). There is no need to question his procedures to that point. It is in compensating for the limited documentary material suitable for psychohistorical analysis that the author's techniques require examination. Again it is best to allow him to provide his own explanation. "In transcending these difficulties, the psychohistorian draws upon the fullness of his own life experience. . . . Many of the insights of this book . . . reflect not so much psychoanalytic theory as the personal experiences of the author" (p. 11).

Any historical work is influenced to some degree by the subjective judgments of the researcher. Yet substituting personal experience for adequate theoretical underpinning—particularly in psychohistory, where unconnected snippets of data are given coherence only by substantial and comprehensive theory—is dangerous. It creates a strong possibility the study will have no meaning beyond that generated in the relationship between biographer and subject. Still, Carroll's life of Samuel Johnson is not a meaningless study. It is a clear, concise, perceptive, and scholarly biography. The other Johnson's Boswell has achieved all this by avoiding many of the problems of his own methodology. He has produced an account in a generally traditional pattern and allowed psychology only an intrusive rather than an essential function. The resultant combination works well.

Carroll's stated wish is to have his study speak not so much about Johnson as about ourselves. He has not achieved this aim, but he has written an admirable monograph. If we have not learned much about ourselves, we have considerably enriched our knowledge of early Connecticut, eighteenth-century American Anglicanism, and one man's struggle, first with his own conscience and then with the consciences of his family, friends, and fellow colonists.

B. R. BURG
Arizona State University

CORNELIUS P. FORSTER. *The Uncontrolled Chancellor: Charles Townshend His American Policy*. Providence: Rhode Island Bicentennial Foundation. 1978. Pp. xv, 155. \$9.95.

The most overworked word in this volume is "probably," and the recurrent use of that modifier reflects

the difficulties a historian encounters in attempting to explain Charles Townshend. "Capricious" is an inadequate description of the man. He changed parties as often as he changed posts, and his correspondence alternately praises and condemns virtually everyone with whom he came in contact.

Cornelius P. Forster focuses on the last six years of Townshend's career, from the time he achieved ministerial rank as Secretary of War in 1761 until his death in 1767. In seven brief and generally well-written chapters, Forster explores Townshend's struggle for political power and his program for the colonies.

Forster argues that despite Townshend's "propensity to fluctuate" (p. xiii) he was consistent on two points. He unwaveringly pursued his own aggrandizement and he advocated a comprehensive imperial program to ensure the supremacy of the mother country. Townshend's decision to accept or reject a governmental position reflected his evaluation of a particular government's staying power and his desire for leadership in the House of Commons. With reference to the colonies, Townshend is pictured as working to free colonial officials from their dependence on local assemblies by paying them from funds raised through parliamentary taxation.

Although Forster has done extensive research in primary materials, he fails to present a clear picture of Charles Townshend. The major portion of the book, despite its title, is a tedious description of Townshend's quest for political power. To argue that there is consistency in pursuing one's own advancement is confusing at best; here it seems to mask confusion about what Townshend is doing or why. He appears now and then in a murky narrative of British politics, but we end up knowing little about him except that he is a brilliant speaker and a political trimmer. We rarely see Townshend attempting to justify his behavior or even to explain it. Moreover, his relationships with such major figures as Pitt, Newcastle, Rockingham—even his brother George—often appear inexplicable. He seems not simply unprincipled but erratic. Why?

If we assume that Townshend was consistent only in pursuing his own career, what explains his purported devotion to a single colonial policy? Were his proposals for America the result of political expediency or did he, in this one instance, have some deeper loyalty? Little attention is given to Townshend's colonial policy and much of what is discussed (the taxation acts of 1767) has been previously explored by Robert J. Chaffin. Forster asserts that Townshend had a "comprehensive imperial program" (p. xiii), but he does not give a clear overview of that program or attempt to explain Townshend's motivations. It might also be argued that Townshend's colonial policy was neither consistent nor comprehensive.

In short, Forster does not tell us much about Townshend, about British politics, or about American policy that we did not already know. The fault may well be in the intractability of the materials rather than in the author, but the book is a disappointment.

DAVID L. AMMERMAN
Florida State University

MARÍA PILAR RUIGÓMEZ DE HERNÁNDEZ. *El gobierno español del despotismo ilustrado ante la independencia de los Estados Unidos de América: Una nueva estructura de la política internacional (1773-1783)*. (Trabajos monográficos sobre la independencia de norteamérica, number 3.) Madrid: Ministerio de Asuntos Exteriores. 1978. Pp. 338.

María Pilar Ruigómez de Hernández has prepared a monograph on Spain and the American Revolution. Unlike previous studies that argued that Spain clearly attempted to help the British colonies out of some noble motive, this book looks at the broad nature of international affairs in the 1770s and early 1780s and the role Madrid and the colonies in North America played in that scheme of events. Thus the book is more of a general history of Spanish foreign policy of the period than a specific analysis of Spain's early relations with the United States.

The book is divided into two sections. The first deals with international political and economic rivalries, especially those among France, Britain, and Spain during the eighteenth century, both from an intellectual and political perspective. The second section has a number of chapters devoted to the American Revolution, involving Franco-Spanish negotiations, a section on Spanish-American contacts, the decision to help the colonies by Spain, and finally on the whole question of the western boundaries in North America. There is no concluding chapter.

The book explores more clearly than earlier studies the role of domestic events on the development of Spain's foreign policy and offers considerable detail regarding the role of individuals and the effect of their political philosophies on diplomacy. The author, however, says little that is new. Archival research is used to discuss Spanish-American relations and contacts with Paris, although most of this material has already been mined by other historians. Bibliographic references suggest that none of the literature on the general subject of late eighteenth-century diplomacy published after 1970 was consulted. The author argues, on the basis of research conducted, that the question of territorial boundaries in North America was extremely important to Madrid, more so than even Gibraltar, and that ri-

valry between Madrid and Paris was reflected in their mutual distrust and animosity toward London when dealing with the New World. Considerable attention is paid to the roles of Aranda and Florida-blanca in developing Spanish foreign policy, an issue that previous historians have glossed over.

The study is too simplistic in its arguments and is poorly researched. No French, British, or even Spanish colonial archives were consulted. The author has, however, provided a balanced view of Spanish foreign policy in the 1770s that will appeal to the serious general reader but not necessarily to the specialist of eighteenth-century diplomacy.

JAMES W. CORTADA
IBM Corporation

A. A. FURSENKO. *Amerikanskaia revoliutsiia i obrazovanie SShA* [The American Revolution and the Formation of the U.S.A.]. Edited by V. I. RUTENBURG. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1978. Pp. 414. 1 r. 90 k.

A. A. Fursenko, a senior member of the growing band of Soviet Americanists, has already produced a series of works in the field devoted mainly to later periods but including *The American Bourgeois Revolution of the Eighteenth Century* (1960). This present work is more than a rehash of its predecessor, even though it falls into that category designated for Soviet readers as "popular-scientific,"—based exclusively on published sources and assuming no great prior knowledge of the subject. Fursenko has been able to make good use of a stay at the Woodrow Wilson International Center for Scholars to develop a close acquaintance with much recent writing on the subject. Thus, if there are inevitably references to the Old Left of Herbert Aptheker, Eric Foner, Philip Foner, and W. Z. Foster, there are also citations of the New Left of Staughton Lynd and especially of Jesse Lemisch. And Fursenko demonstrates wide reading among other schools of domestic scholarship on the American Revolution, not surprisingly finding more to agree with in the pages of Merrill Jensen and Jackson Turner Main than in those of Bernard Bailyn and Edmund S. Morgan. None of these scholars would be advised to seek a translation of the book unless they wish to ascertain that their views have not been misrepresented, for they would learn little if anything from it about the area of their expertise. Indeed, a warning might more generally be issued to all potential American readers, none of whom would be likely to need the sometimes very simple accounts of such milestones on the road to revolution as the Stamp Act crisis and the Townshend Acts.

Nevertheless, the book has some interest as a well-written presentation of the American Revolution

and the debates surrounding it simply because it has been achieved by a Soviet scholar. It clearly indicates that Soviet writing on its subject has come of age and that Soviet readers are no longer obliged to accept the crudities that were offered them just a few decades ago. True, they are warned off the "consensus" school of interpretation and are urged to see the inconsistency of those who simultaneously preach the exceptional and the universal nature of 1776 and its sequel. Moreover, they are discouraged from giving their acceptance to the Atlantic interpretation of the American Revolution, that is, from placing it in anything like the same category as the French. Yet these positions have all been taken up by at least some reputable American historians in recent years, and Fursenko is aligning himself with them rather than dismissing all non-Marxist American scholarship as "bourgeois falsification." In addition, there is in his work from time to time a spirit of enthusiasm for at least some moments in the great drama and for at least some of the actors. He appears genuinely stirred by the Boston Tea Party and the "shot heard around the world" as well as by Benjamin Franklin and Patrick Henry, in a manner that those brought up on such events and people might find difficult to recapture. At the end, however, and not surprisingly, Fursenko takes issue with Richard Morris about the relative significance of the American and Russian Revolutions.

PAUL DUKES
University of Aberdeen

GERARD W. GAWALT. *The Promise of Power: The Emergence of the Legal Profession in Massachusetts, 1760-1840*. (Contributions in Legal Studies, number 6.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 254. \$19.95.

Whereas ministers were the dominant professionals in colonial America and doctors attained that eminence in our century, the nineteenth century belonged to lawyers. Gerard W. Gawalt traces the structural transformation of the Massachusetts legal profession as it rose to power in the early nineteenth century. He provides informative data on lawyers' social backgrounds, career patterns, wealth, income, and legal training and on the political struggles of leading lawyers to maintain professional autonomy.

Gawalt's interpretation of this data is very much in keeping with the recent work of legal historians Richard Ellis, Morton Horwitz, and William Nelson. According to Gawalt, the growing commercial economy after the American Revolution required lawyers' skills, thus stimulating a dramatic increase in the number of lawyers. Lawyers became competitive and individualistic, shedding the corporate-minded orientation they had before the Revolution.

Ironically, as the profession became less cohesive and more stratified, it became more powerful.

The Promise of Power also builds on the revisionism of Maxwell Bloomfield by providing a detailed refutation of the traditional view that the 1830s marked the nadir of professional development, when an uninformed egalitarian spirit undermined professional standards. Gawalt shows that the power of lawyers grew despite attacks by antilawyer and antielite reformers and despite the occasional passage of legislation designed to democratize the profession and undermine the power of legal institutions. As a refutation of earlier interpretations, Gawalt's study is convincing. He shows that, although formal bar admission standards were lowered in the 1830s, entry to the bar remained restricted by informal barriers. Also, leading lawyers were able to blunt or coopt most reforms. They were able to do so in part because the legal profession had established a virtual monopoly over the judiciary and had become a significant voice in the state legislature and politics generally.

As an exploration of the ramifications of lawyers' power, however, the study is less satisfying. A full analysis of power should explore more than lawyers' fees, access to public office, and control over training and over entry into the profession. How did lawyers perceive the role of law and their profession in the emerging commercially oriented society? Did they have a guild mentality, or did they think of themselves as atomistic individuals? Were they in any any sense class conscious? How, for example, did they define their relationship with other professionals and with the new commercial elite? Gawalt provides suggestive bits and pieces of evidence about all these matters but explores none of them in depth. *The Promise of Power* should stimulate studies of other states. Hopefully, such studies will not only build on Gawalt's excellent base, but will also examine the culture of professionalism and the full implications and ramifications of lawyers' power.

WAYNE K. HOBSON
California State University,
Fullerton

WILLIAM L. VAN DEBURG. *The Slave Drivers: Black Agricultural Labor Supervisors in the Antebellum South*. (Contributions in Afro-American and African Studies, number 43.) Westport, Conn.: Greenwood Press. 1979. Pp. xvii, 202. \$16.95.

Rather than an analysis of drivers *per se*, this revisionist study concentrates on what various observers (historians, planters, travelers, exslaves) have said about them. An appendix provides an interesting and useful essay on the evidence. Thoroughly re-

searched in both primary and secondary sources, *The Slave Drivers* is the most thorough statement on the topic to date.

The historical reputation of black slave drivers (bondsmen who managed slave laborers) has suffered from neoabolitionist interpretations that stereotype drivers as barbarous half-savages so debased by slavery that they found sadistic pleasure in inflicting pain on their hapless brothers in bondage. William L. Van Deburg disputes this depravity thesis and argues that while the drivers may have served their owners' economic interests, it does not follow that they were insensitive to the needs of the slave community. Their responsibilities included directing labor in the fields, maintaining discipline, time- and account-keeping, guarding the crops, distributing weekly rations, and more. The author draws an analogy between drivers and modern shop foremen who must satisfy the employers' demands while articulating the concerns of the workers for whom they are responsible. He believes, however, that the driver "most often sides with 'labor' as opposed to 'management,'" and concludes that "the pressures of possessing both instrumental and expressive modes of leadership did not destroy the driver psychologically or turn him into a sadistic oppressor of his fellow bondsmen" (p. xiv).

Actually, there were no universally recognized traits that identified the slave driver. They were young and old, large and small, servile and fiercely independent, brutish and humane. While some flogged slaves without mercy, others assisted bondsmen by faking whippings, covertly distributing extra rations, fighting with violent overseers, and behaving in other ways that were decidedly not in the master's interest. In doing so, they "sought to preserve their integrity" by refusing to become "psychologically devastated brutes" (p. 115). The key to understanding these so-called "privileged" bondsmen lies in the realization that drivers had their roots in the slave quarters. They had stronger emotional bonds with fellow slaves than with masters and, as members of the slave community, were part of its family, religious, and social life. The core of their identity lay in the quarters, which provided them with the psychological protection to withstand the forces of dehumanization that might have rendered them into brutes.

If a reservation can be lodged against this book, it stems from the paucity of primary sources on slave drivers, which in turn forces the author to strain the evidence to support his thesis. The chapter dealing with the perceptions of travelers in the South, for example, is based on only six accounts. This is beyond Van Deburg's control, of course, for there probably are no caches of material on the drivers to be unearthed. Nevertheless, the author has used the available materials with dexterity and imagination

and has shed considerable light on a little-understood phase of slave life.

RONALD L. LEWIS
University of Delaware

ROBERT J. BRUGGER. *Beverley Tucker: Heart over Head in the Old South*. Baltimore: Johns Hopkins University Press. 1978. Pp. xvii, 294. \$15.00.

In this interesting biography of Nathaniel Beverley Tucker (1784–1851), Robert J. Brugger has recounted a life of unusual variety and chronicled opinions the more absorbing for being lost to us in this democratic age. Tucker was a man of Virginian habits and inclinations, opinions and prejudices. Allied to the Randolphs and himself the son of that distinguished gentleman and jurist St. George Tucker, Beverley was an excitable and ambitious young man who lacked discipline and good luck and never succeeded, after having begun in a dozen ways to make his mark, in making his mark stick. Brugger shows him pursuing his studies in a clamorous and diminishing Williamsburg, braving and failing at the practice of law in Southside, Virginia, and finding the conditions of life there increasingly irritating to his pride and mortifying to his hopes of independence and prosperity. Always shadowing this young man was the stern standard of his father—indeed, of that Founding generation that his father represented.

Like many another unlucky gentleman, Tucker “lit out” for Missouri, where he lived for many years prospering and at times gaining public esteem and judicial office. Among Tucker’s most intriguing projects in those years was his hopeful and nostalgic attempt to establish at Dardenne Creek a neighborhood of likeminded settlers, a “slaveholders’ Camelot.” Tucker and his slaveholding friends were to live a life of leisurely and measured elevation and occupation, patriotic and distinguished. This vision of a nostalgic alternative to the contemporary democratic confusions appealed to and informed Tucker’s vision of the world around him. He looked for that gentility and order that would be his proper setting and that would accord him the deference and inner satisfaction he seems not to have gained from his family or from the world and that he continued to consider his due. Tucker was an unhappy man, dissatisfied and, as Brugger emphasizes, at ever-increasing distances from the commonly perceived reality of his countrymen.

Returning to Virginia in the wake of Nullification and the threat of Jackson’s nationalist fervor, Tucker succeeded to his father’s place as professor of law at William and Mary and became preceptor to a generation of Virginia law students and quarterly readers. In many respects he followed his ec-

centric half-brother John Randolph in his opinions and in his ambitions for an independent southern future modeled on the Virginian past. He wrote two novels, *George Balcombe* and *The Partisan Leader*, which sought, one pastorally and the other politically and apocalyptically, to make clear in fiction those standards and possibilities Yankee democratic life in America was obscuring. Tucker spent his last years dizzied and depressed by the hope of making his conservative-romantic and secessionist views count for practical purposes.

Brugger describes especially well Tucker’s frustrations as a young lawyer and his experiences as a Virginian in frontier Missouri. He presents Tucker’s ideas clearly and tries to give back to them some of the force the intervening and, from their point of view, calamitous history has taken from them. In one sense this is the story of a child of a Revolutionary father stymied and infuriated by the burdens of his inheritance. Brugger is careful not to strain our sense of Tucker’s importance. He presents a man whose opinions, and emotions, and experiences reflected those of his southern brethren. This is argued in an afterword, which, if it does not altogether establish Tucker’s representativeness, does show how certain of the opinions he advanced were common southern notions, although this sharing is noted rather than explored. For a book that presents itself as psychohistory, the discussion of Tucker’s character, personality, and family relations is less substantial than the rather elaborate methodological observations on the subject lead one to expect. The book’s tone is distant, as is its portrait of Tucker the man. In short, this book gives a good account of Beverley Tucker and rescues him from neglect.

ROBERT DAWIDOFF
Claremont Graduate School

VIRGINIA BERGMAN PETERS. *The Florida Wars*. Hamden, Conn.: Archon Books. 1979. Pp. 331. \$22.50.

Between 1810 and 1858 the United States fought a series of nasty little wars in Florida, which have been largely forgotten by Americans, and it is not difficult to understand why. These were wars that were fought between a large force of Americans, with great wealth and technological superiority, and a small force of fugitive Indians and blacks whose wealth and technology consisted of no more than what they could cultivate, scavenge, and steal. They were wars that were fought in a tropical area, then largely unknown to American scientists—an area that inflicted terrible punishment on the American military. They were wars in which a massive and expensive American military presence was made to look foolish by a handful of ethnic adver-

saries who fought so valiantly that many American soldiers came to admire them and said so. They were wars that, as they wore on and on, led to moral revulsion on the part of an important segment of American society.

The similarity between the Florida wars and the Vietnam war is obvious, and Virginia Bergman Peters touches on this similarity more than once. Apart from this, she has little to say about the Florida wars that has not already been said. Her main contribution in this book is that she pulls together, within the covers of a single volume, the entire, sad sequence of events. As such, her book is a readable synthesis of a fairly substantial literature.

It is a literature that is rich in the stuff of which novels are made. A heavily armed fort at the mouth of the Apalachicola River manned by escaped black slaves; a society composed of southern Indians and blacks whose loyalty to each other was so fierce that the American military had to batter them to a bloody remnant before any of them could be forced or persuaded to be informers or turncoats; a black interpreter, Abraham, who negotiated for the Indians with polished manners and "with a countenance which none can read" (p. 141); an Indian leader, Halleck-Tustenuggee, captured by the Americans under a white flag, accused by one of his men of *wanting* to be captured, who leaped up and knocked the man to the ground by striking both feet onto his chest, and then bit his ear off, ground it in his teeth like a mastiff, and spat it to the ground, shouting "Tustenuggee . . . Halleck-Tustenuggee!" (p. 247).

The shortcomings of Peters' book are the shortcomings of the previous scholarship on which it is based. Two fundamental questions beg for answers. How did the Indians and blacks maintain discipline and morale in the face of such terrible odds? That is, from what ideological wellsprings, from what social experience, and from what vision of the future did their actions proceed? And an even more fundamental question is how did this series of wars fit into the history of the American social and economic system? Why, in retrospect, do these wars, as does the Vietnam war, seem so grotesque, so seemingly aberrant? Such questions make it clear that what Frances Fitzgerald's *Fire in the Lake* did for the Vietnam war needs to be done for the Florida wars.

CHARLES HUDSON
University of Georgia

JOHN MACK FARAGHER. *Women and Men on the Overland Trail*. (Yale Historical Publications, Miscellany, number 121.) New Haven: Yale University Press. 1979. Pp. xiii, 281. \$17.50.

The title of this volume is misleading. This is *not* a study of the overland trail. Rather, it is a study of

"the relationship between men and women in marriage in the mid-nineteenth century Midwest" (p. 3). The stage on which John Mack Faragher examines this relationship is the overland trail, 1840-70. Although Faragher contends that "the trail experience was no more remarkable than ordinary family life and struggle," he also acknowledges that, "for most of the emigrants the trip was a spectacular event in their lives, unlike anything they had done before" (pp. 4, 11). And, as Faragher describes the difficulties of the journey, it is clear that the trail experience was unique and that it put unusual strains on family relationships. The key to this seeming contradiction is that Faragher is not so much interested in family relationships on the trail as he is in examining family life in a small segment of society at a particular time—the midwestern farm family just prior to the coming of modernization and commercialization. The overland trail is a convenience necessitated by the lack of adequate source material of the type Faragher needs, specifically, diaries by both men and women. Since midwesterners wrote little about daily farm life, Faragher is forced into the difficult position of trying to use the available descriptions of an extraordinary experience, the overland trail, to reflect what the relationships between these people must have been like before their journey.

In the course of this historical balancing act, Faragher falls into several contradictions and errors. Almost every sentence is loaded with value-laden words and phrases that depict women as "exploited," "subordinate," and "powerless." Writing from a Marxist orientation, Faragher defines all relationships in terms of the class, race, and sexual struggle, and within this struggle Faragher sees women as victims of a patriarchal society dominated by masculine attitudes of superiority (pp. 15, 62). To prove his points, Faragher makes statements that are not supported by his sources or are misleading as he uses them. For example, Faragher writes that trail women rose before the men to stoke the fires and milk the cows (p. 76). His support for this statement is a diary by Helen Carpenter that taken in context, says "some women" had these responsibilities and that most of those who did were "Missourians." Faragher dismisses this by saying that most women "were from Missouri and its midwestern environs" (p. 80). But "Missourian," as Faragher well knows, was a trail term used to designate a particular class of people, usually down-and-outers. If the evidence does not fit his preconceptions, Faragher simply discards it. On page 87 he uses a quote from Phoebe Judson to illustrate a "divergence of the sexes" and, in a neat piece of juggling, accepts her statement as to her emotional state but rejects her comments about her physical well-being.

Despite these flaws, there is much that is right about the book. Faragher's analysis of midwestern family life and his provocative use of new social science techniques raise exciting possibilities for further research. Faragher has borrowed liberally from social psychology, sociology, folklore, and demography. He is not very sophisticated in applying these new methodologies, but he has shown how they might be utilized in developing new approaches to the study of regional history. This book will excite a good deal of comment, and it should generate a number of useful new studies.

SANDRA L. MYRES
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THOMAS A. BRYSON. *An American Consular Officer in the Middle East in the Jacksonian Era: A Biography of William Brown Hodgson, 1801-1871*. Atlanta: Resurgens Publications. 1979. Pp. xiii, 214.

Thomas A. Bryson's biography of William Brown Hodgson is part of a growing body of scholarship about the early American experience in the Middle East. The precocious Hodgson was sent to Algiers as a language student by President John Quincy Adams in 1826 because the United States government was so devoid of expertise about the area that letters received in Arabic could not be translated. After a three-year apprenticeship under Consul William Shaler, Hodgson became one of that handful of individuals in nineteenth-century America who tried to pursue a professional career in an unprofessional foreign service, where place and advancement depended less upon merit than upon political connections. After working as a clerk-interpreter at the Department of State, in 1831 Hodgson became dragoman to the American legation at Constantinople. In 1833, however, he was unceremoniously fired by Commodore David Porter, the ill-tempered retired naval officer whom Andrew Jackson had appointed to represent the U.S. to the Ottoman Empire. Bryson uses the unseemly feud between the nepotistic minister and the pretentious dragoman to assess the glaring weaknesses of the foreign service in the age of Jackson. Because the politically astute Hodgson had been able to enlist an unlikely collection of patrons that included John McLean, Martin Van Buren, and Daniel Webster, he continued to receive occasional appointments from 1833 to 1840 as special agent to Egypt, Morocco, Peru, and Prussia. Hodgson finally achieved the position he had worked for most of his life when President Tyler made him consul to Tunis in 1841, only to resign a few months later in order to marry a wealthy heiress, Margaret Telfair of Savannah, Georgia. Thereafter, Hodgson lived a life

of ease devoted mostly to studying the languages and customs of the peoples of Africa and the Middle East. Without the benefit of a formal college education, Hodgson earned an international reputation as a philologist and mastered nearly a dozen languages including Arabic, Berber, Persian, and Turkish.

Drawing primarily on the records of the Department of State and the Hodgson papers at the Georgia Historical Society, Bryson focuses on Hodgson's career as a consular officer from 1826 to 1842. Bryson's research is thorough, his judgments balanced, and he ably places Hodgson in the context of the complicated international politics of the Mediterranean world. Bryson, however, is not much of a stylist, and one wonders whether it was necessary to record every stop Hodgson made on a trip from Washington to Callao, Peru, and back in 1837-38 as a bearer of dispatches. It also may be doubted whether Hodgson merited anything beyond a biographical essay. The most important contribution of the book lies not in the biography of Hodgson himself but in the insights into the amateurish foreign service provided through Bryson's thoughtful analysis of Hodgson's consular career. The author has written a useful study of one of those little-known men who "deserve their place in history, for their enterprise provided this nation with its entrée into a remote region of the world, often under the most difficult of circumstances" (p. 5).

KENNETH E. SHEWMAKER
Dartmouth College

ROBERT E. LEVINSON. *The Jews in the California Gold Rush*. (Landmarks of Western Jewish History, number 1.) New York: KTAV Publishing House. 1978. Pp. xvii, 232. \$15.00.

The ships that brought gold seekers of many nations to California beginning in 1850 carried a conspicuous number of recent German Jewish immigrants. Most of them set up in business in the mine towns after peddling among the camps, while some were actually miners. Their life as businessmen and local worthies and as Jews in the gold fields from about 1850 to 1880 forms the subject of Robert E. Levinson's interesting work. As he demonstrates, they did well, thanks not only to their skill and probity as merchants but also to the advantage they enjoyed of having good connections with the Jewish wholesale houses in San Francisco that were their suppliers. Besides, the miners were eager customers. The German Jewish immigrant merchants constituted the great majority of the commercial class in the gold fields during their heyday.

A few of the merchants were prosperous enough to invest in the expensive pit mining enterprises

that replaced the worn-out surface mines. Few merchants were that wealthy, but they were men of substance, active in the rudimentary social and civic life of their towns. Their religious observance was casual, although most were nominally Orthodox. Yet they erected fine synagogues (which they seldom attended) and equipped them handsomely. As the years passed, however, the mining towns settled down from turbulent frontiers of expansive consumption to placid, rather self-sufficient agricultural backwaters. Then the Jewish settlers tended to drift away, mainly to San Francisco. Synagogues were closed and cemeteries abandoned.

All this Levinson tells us fully, with exhaustive documentation from obscure local sources supplemented by interviews with numerous descendants of the settlers. He has provided fine illustrations and an elaborate index. However, not all his abundant material is effectively integrated into the text. It was regrettably thought essential to list financial sponsors in front, a page each for "Honor Historians," "Patrons," and "Sponsors." *The Jews in the California Gold Rush*, however, is no filiopietistic tract, notwithstanding a light apologetic tone here and there (for example, pp. 38, 68).

Levinson is not always successful at the interpretive level. Was there anti-Semitism—if that term applies then—in the gold fields? He stresses that the Jews were freely and fully accepted in local society (pp. 62, 88, 101). Yet he devotes fifteen pages to anti-Jewish expressions in the local press. Most of it emerges as earthy jokes or rough and ready comments on distant matters. Two actual incidents appear insignificant. Above all, the Jews were a very visible and distinct group, and comments on them seem to lack deep hostility or venom.

The author stresses that his merchants' business trips to San Francisco were also occasions for matchmaking in that large Jewish community. A good match also meant improved commercial connections, as had been true for centuries of mercantile history. However, we hear nothing of family life in the gold field towns.

Levinson's carefully detailed book is pleasant reading and is a worthwhile contribution to American Jewish and regional history. One wishes Levinson had deployed some of his rich material for comparative and somewhat deeper analysis.

LLOYD P. GARTNER
Tel-Aviv University

ROBERTA BALSTAD MILLER. *City and Hinterland: A Case Study of Urban Growth and Regional Development*. (Contributions in American History, number 77.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 179. \$17.50.

As Roberta Balstad Miller correctly points out at the beginning of her book, "the study of American urban growth as an integral part of the development process of the surrounding region and, in turn, as a critical influence upon that region has rarely been attempted" (p. 3). The implication that this lacuna in American urban history is important is also correct, for it is in the context of developing city-hinterland relationships that we can best understand the role of the city in the social and cultural life of a nation that remained largely rural through most of the critical stages of its growth. Thus, the appearance of a book that the author describes as "a case study of urban growth and regional development" could be expected to arouse the deepest interest of urban historians, especially since the locus of the study is the region of a growing inland city (Syracuse, New York) during the critical decades of urbanization and national expansion that preceded the Civil War.

Unfortunately, *City and Hinterland* does not rise to its own challenge. Though useful for what it achieves (basically, a delineation of the effects of sequential transportation developments on certain economic and demographic shifts within a narrowly defined hinterland), it is too narrowly conceived to suggest but a fraction of what can be gained from systematic study of developing urban regions. Missing is the articulation of functioning institutions and communities, set within the physical space and specific terrain of the developing region, and missing as well are many of the issues that a broader and more complex study might have raised—for example, one gets no sense of the cultural significance of the city in its hinterland or of the tenor or even the frequency of rural-urban contact. Still more disturbing is the failure to make the most of those aspects of regional development on which the work does focus. No mention is made of central place theory; yet the entire study could have been conceived of and presented as an attempt to provide the dynamic, developmental component that this fascinating geographical construct still lacks—the data are assembled, but they serve only more modest ends. Finally, one of the author's central conclusions, that "urban growth had unfortunate consequences for the city's hinterland" (p. 157), is contradicted by her own data, which show that competition from the more fertile farmlands of the Midwest, not the growth of Syracuse, drained rural Onondaga County of its profits, its capital, and many of its people. The growth of Syracuse, indeed, was the one thing that continued to sustain the life of the hinterland. Miller should have challenged the idea of "urban dominance," rather than reinforce it.

The contribution of this book, then, is that it alerts us to a field of study that has been badly ne-

glected by urban historians. It is my hope that it will inspire others to go beyond it to more fully developed analyses of cities in their regions.

STUART M. BLUMIN
Cornell University

CARL SIRACUSA. *A Mechanical People: Perceptions of the Industrial Order in Massachusetts, 1815-1880*. Middletown, Conn.: Wesleyan University Press. 1979. Pp. 313. \$20.00.

It is possible, Carl Siracusa assumes, to get at Massachusetts opinion regarding industrialization and the working class through the public statements of the state's leading politicians, men who presumably told the public what it wanted to hear. In fact, they said very little on this subject, which encourages Siracusa to move in two interesting directions. First, he pieces together what fragmentary references he can find to reconstruct the image of the "respectable worker." This rather vague and superficial image, he argues, dominated political discussion and apparently blunted serious criticism, though he acknowledges that after mid-century the labor movement forced the "labor question"—if only in a limited way—into political discourse.

The simple fact of this ideological impoverishment constitutes his second concern. He correctly asks why there is so little evidence of genuine critical discourse in Massachusetts politics. What made the case of industrialization in Massachusetts so different from that in England, where questions concerning the condition of the working classes moved rather quickly into the political arena? It is a fundamentally important question, but I do not think he satisfactorily answers it. He looks to the history of the economy (for example, milder American conditions, the absence of competing economic interests) and general cultural "values" (for example, Yankee materialism, nationalistic optimism) for his answer. Surely part of the answer is there, but it is striking that he does not look also for a political explanation—one that explores the social history of power, political institutions, and political discourse. Although suggestive material abounds in his narrative, he seems unable to phrase the question in a way that leads into the political realm, into the comparative character of the two political cultures. How did issues in England and Massachusetts become a part of political discourse? Moreover, Siracusa defines political rather narrowly: forty-two very elite and mostly Whig officeholders, two party newspapers, and not much more. Perhaps the realm of political discourse is in fact wider.

The book's organization is not entirely satisfactory. In part two, where Siracusa reconstructs the consensual image of the "respectable worker," the

period from 1815 to 1865 is treated as a whole with little sense of change over time; statements from a variety of speakers from any year in this dramatically transformed half-century are brought together to form a single composite. When we get to part three, where some critics are introduced, we find an overlap of dates with the earlier positive consensus of twenty-five years. This causes some conceptual problems he might have worked through. (It should be said, however, that the third section itself is better in relating opinion to historical change.)

For this sort of study, precise meanings must be given to (and taken from) words like manufacturing, factory, factory system; worker, laborer, wage earner, operative, working class; commercial and industrial. The changing patterns of usage—along with the historically evolving sociology attached to them—make these terms vexing for the historian trying to extract meaning from texts and in choosing his own language. Siracusa seems to realize this, but the relation between these terminological matters and his thesis is so close that I wish he had faced it more fully and directly.

THOMAS BENDER
New York University

GEORGE B. FORGIE. *Patricide in the House Divided: A Psychological Interpretation of Lincoln and His Age*. New York: W. W. Norton. 1979. Pp. x, 308. \$14.95.

Freud was nine when the American Civil War ended. It was not until 1979 that the two had a truly substantial meeting. It has been a long time but the wait was worthwhile.

In searching for the origins of the Civil War George B. Forgie combines the analytical techniques of psychohistory, American studies, and traditional history. His thesis is that "a [the?] central dilemma" of the antebellum generation was the fame of the revolutionary fathers, which made their "sons" (Clay through Douglas) members of a "post-heroic cohort": men whose ambition was thwarted by the immortality of their sires. The sons' consequent parricidal urges were balanced by their filio piety; they rebelled but in an unconscious manner. In the 1840s Young America's commitment to an illusion of boundlessness—illustrated most dramatically in manifest destiny—was a failed rebellion. So was the Kansas-Nebraska Act. Douglas, the chief rebel, "inflicted a great wound" on the fathers' tradition of compromise on slavery but was "killed" by Lincoln. Between the twain, and in the face of the parricidal tendencies, some advocated sentimental regression from politics to domesticity in the fathers' house of the early Republic. But there is no escape into childhood, and the sentimentalist flight helped bring on the Civil War by liberating "the

fratricidal impulses of the post-heroic generation" (p. 198).

In the 1830s the "killer" of Douglas, among others, prophesied a fratricidal conflict. The prophecy was self-fulfilling, and in the 1850s, in a melodramatic ritual, Lincoln made Douglas play the scapegoat: the bad son threatening the fathers' house to be "killed" by the good son. Lincoln could thus both save the fathers' house and match their immortality. The projection of unrecognized parricidal urges onto Douglas, however, created tensions that Lincoln could not control. The war came (and thus Forgie provides the greatest leap of faith in his schema) and with it the birth of a modern nation destroying the fathers' Republic forever. Such conceptualization gives Lincoln a gigantic role but he was able to play the role because his generation "shared the conflictual emotions he contained within himself and acted out for them" (p. 284). In the end, having long ago transferred his filial affections from his father Thomas to the fathers of the nation, Lincoln abandoned them to become "the obedient and ultimately sacrificial son" of "our Father in Heaven" (pp. 33, 288).

To readers who can accept the legitimacy of Forgie's approach—though they may disagree with much, as I do (for example, the delineation of both Lincoln and Douglas)—*Patricide* carries a precious grain of truth and therefore is an important book. To others it will only engender bewilderment—in Turgenev's words "The courage to believe in nothing." Among the former is the jury of the 1972 Allan Nevins Award.

Forgie develops his clear, coherent argument with a sure hand. Many caveats are nonetheless in order and at least a glimpse of some is needed here. The Freudian theoretical framework is, of course, vulnerable—but so is any theory. Within that framework Forgie minimizes the first half of the *anschaung* "lieben und arbeiten"—to love and to work—to the point where he dismisses the absence of an American Jocasta in his Lincoln-Oedipus analogy by reminding us that "Oedipus got more than his father's wife; he also got his father's job. Moreover, the wife came with the job . . ." (p. 285). At times Forgie is quaintly old-fashioned both in his theory and its specific applications. He even speaks of a "maternal persona" (p. 167). But Lincoln's mother—surely an all-important influence—is not mentioned.

Forgie makes no attempt to place his work in the structure of previous historical literature, leaving, perhaps wisely, the job of integration to his colleagues. It should be noted, however, that if we accept the metaphor of parricide, Forgie's work, too, must be seen as an act of parricide, above all toward his mentor Don E. Fehrenbacher (and also David M. Potter), for this is "a book on the origins

of the American Civil War that deals hardly at all—and then indirectly—with the sectional conflict, slavery, or political parties . . ." (p. ix). Indeed, predecessors in general are excessively, though obliquely, in target. Marvin Meyers, for example, should not have been left out of the footnotes. Edmund Wilson deserved more credit.

Forgie's reasoning wobbles in places: the idea that Douglas did much to destroy the fathers' peace contradicts in part—for the historian if not the psychologist—the notion that he was a mere scapegoat. A single paragraph on page 82 contains three statements indefensible in the face of chronology. The crucial use made of seemingly unimportant words—slips of tongue perhaps—is acceptable if we accept the psychoanalytic framework; but the indifference to context with which Forgie sometimes uses these words is not (for example, p. 273 n. 56). Most significantly, the import postulated for the past in the American mind will not be easy to credit for students of comparative history: Americans are a profoundly ahistorical people.

But no matter. Forgie's work deserves not only attention but extension (and thus testing). After all, Washington's generation—the fathers—were also the children of fathers, and the children were also the fathers of children. Is parricide a perennial factor in historical change? And, if so, can it be demonstrated? Some years ago Jacques Barzun lamented, "It is so easy to make what looks like a book, by taking notes instead of thought and by following an outline instead of an inspiration. Everything—time, current practice, and even our scholarly virtues—tempts us to imposture." It is a pleasure to report that Forgie is no impostor.

GABOR S. BORITT

Memphis State University

SANDRA S. SIZER. *Gospel Hymns and Social Religion: The Rhetoric of Nineteenth-Century Revivalism*. (American Civilization.) Philadelphia: Temple University Press. 1978. Pp. xi, 222. \$15.00.

This study discusses revivalism in nineteenth-century America. Sandra S. Sizer proposes a "historical sociology of religious language," which is "a perspective which employs insights and methods from several disciplines, especially those varieties of anthropology and literary criticism that emphasize cultural phenomena as linguistic phenomena intimately related to particular social settings" (p. 10). The book's real strength is that it draws the lyrics of gospel hymns (in particular those from the publications of Dwight L. Moody and Ira D. Sankey), novels of the period, the revivals themselves ("the Moody and Sankey fever," one source called them), and other aspects of our history into a relationship that is often illuminating.

Yet, as with many studies that attempt to relate various disciplines, Sizer's work is only partially successful. One of the problems is something that the author has set up intentionally. She does not discuss the music that is an integral part of the gospel hymns, explaining that "our current theories of musical meaning and our histories of American popular music are too sketchy to allow a nonmusicologist to draw conclusions about the relationship of the music to other cultural trends." Three justifications are given for ignoring the music—namely, that the lyrics often existed as poems in their own right, that in the mid-nineteenth century "tunes began to derive their names from the words with which they were associated," and, finally, that "singers and musicians themselves paid a great deal of attention to the words" (p. 9). Of course it is true that the lyrics are interesting in themselves. But the music provides "conclusions" that are important and that would illuminate the points the author is making. It is questionable that current work in American vernacular music is so sketchy that consideration of the music is an impossibility for one who is not a professional musicologist, and the reasons for omitting a study of the music, at any rate, are not credible. The finest studies on opera libretti, for example, rely constantly on the music that was set to words. When the author does venture into brief considerations of musical matters, the result is not satisfactory. Perhaps some of these problems could have been alleviated if the title of the book had been changed to *The Texts of Gospel Hymns and Social Religion*. At least then we would be aware of the book's focus at the onset.

Some of the discussions of hymn texts state the obvious, and it is a pity the author did not venture more critical opinions on the poems and novels she analyzes. Often the discussion gives longer than necessary plot synopses (such as that of Timothy Shay Arthur's *Ten Nights in a Bar-room*). Although the notes are extensive, there is no bibliography.

The overall impression, however, is positive. Sizer reaches interesting and useful conclusions about her subjects, and she tells us much about the "fever" that made revivalism such an American situation and such a product of its time.

GARRY E. CLARKE
Washington College

DEBORAH PICKMAN CLIFFORD. *Mine Eyes Have Seen the Glory: A Biography of Julia Ward Howe*. Boston: Little, Brown. 1979. Pp. 313. \$15.00.

Deborah Pickman Clifford's biography of Julia Ward Howe appraises the life of one of the most well-recognized and admired women of the nineteenth century. During her long and productive life,

which spanned the years 1819–1910, Howe published several volumes of poetry and prose, lectured throughout the United States and Europe, served as an editor of the American Woman Suffrage Association's weekly magazine, the *Woman's Journal*, aided in the forming of women's clubs throughout the country, and became a national celebrity after authoring the "Battle Hymn of the Republic" in 1861. This long list of accomplishments, plus many others, is noteworthy in and of itself. What makes it truly extraordinary is the fact that Howe engaged in these activities while raising six children and tending to the needs of her husband, Samuel Gridley Howe, who voiced numerous criticisms of his wife's work outside the domestic sphere.

Drawing primarily from Howe's personal papers, Clifford does a commendable job of relating how Howe juggled family responsibilities with an active career as a writer, lecturer, and reformer. During her early years of marriage in the mid 1840s, Howe bemoaned the isolation she experienced as an intelligent young woman who felt overwhelmed by the responsibilities of motherhood, while her husband remained away most of the day pursuing his own profession. Although Howe came to enjoy motherhood, she found it much more difficult to reconcile her yearning for a career with her husband's desire that she remain exclusively a devoted wife and mother. Clifford shows how she gradually came to realize that it was necessary to develop her own talents, even if this meant angering her husband. Thus, from the publication of Howe's first book of poetry in 1854, until the death of her husband in 1876, her marriage was wracked with conflict and crisis.

Clifford is less successful in relating the life of Howe to the emerging nineteenth-century woman's movement. For example, in explaining why Howe was not elected to a leadership position in the newly formed National American Woman Suffrage Association in 1890, Clifford suggests that this can be partially attributed to the fact that many suffragists did not feel that she "was single-minded enough about the woman's movement" (p. 250). Unfortunately, the author never fully explores this idea. Because Clifford is evidently unfamiliar with the complexities of the nineteenth-century woman's movement, she misses an opportunity to discuss a very important aspect of the life of Julia Ward Howe.

JUDY BARRETT LITOFF
Bryant College

JOHN R. WUNDER. *Inferior Courts, Superior Justice: A History of the Justices of the Peace on the Northwest Frontier, 1853–1889*. (Contributions in Legal Studies, num-

ber 7.) Westport, Conn.: Greenwood Press. 1979. Pp. xvi, 246. \$18.95.

This little book contains what many of us always wanted to know about justices of the peace but never expected to find in one place. John R. Wunder begins with an encapsulated history of the office from the twelfth through the nineteenth century and then illustrates the principal lines of development with a comprehensive case study of JPs in the territory that is now the state of Washington. Although JPs often exceeded their commissions throughout these seven hundred years, their communities and higher authorities (however various) usually ratified their use of power because they provided necessary and valued services: maintaining law and order and supplying judicious decisions at the local level. An institution that originated in medieval England thus proved equally useful in modern America, and it was part of the cultural baggage that accompanied the settlement of the continent. The author concludes that the myth of lawlessness on successive American frontiers has little basis in fact and that the very prevalence of the office is itself evidence that contradicts the conventional wisdom.

In surveying the existing literature about conditions along the borders, Wunder frequently reaches conclusions different from those of earlier writers. The reason for this variance lies in the sources. Too many accounts have been based upon romantic notions about the wildness of the West or exaggerations of Turnerian theory, but Wunder's is grounded on verifiable data and that is the significance of this monograph. Long after everyone has incorporated the meat of part one in lecture notes, the demonstration in later chapters that mythology falls before evidence should remain to challenge the profession. American legal history is on the verge of maturity: the point where reality replaces romanticism. Wunder has joined a small but growing number of scholars whose microcosmic studies will someday provide the framework for a more accurate view of this important part of our national experience. One hopes that at that point legal history—like other specialized fields—will be incorporated into a more comprehensive understanding of our past.

The book has some oddities of language, style, and organization; a few instances where the author does not follow his own advice to examine primary sources; and some judgments that may not stand upon re-evaluation. In its emphasis upon the legal aspects of the office under federal territorial jurisdiction, it complements Robert M. Ireland's *County Courts of Antebellum Kentucky*, which emphasized the political aspects within a state. Together these

books suggest a fruitful field for comparative studies.

MARY K. BONSTEEL TACHAU
University of Louisville

MORRIS F. TAYLOR. *O. P. McMains and the Maxwell Land Grant Conflict*. Tucson: University of Arizona Press. 1979. Pp. xvi, 365. Cloth \$16.50, paper \$9.50.

Oliver P. McMains was born in Milford, Ohio in 1840. When he was fifteen, his mother died and his father went to California, leaving him in the care of his aunt, Debbie Mount. He learned the printing trade, at nineteen became a Methodist minister, and then served pastorates in Illinois before going to Cimarron, New Mexico in 1875.

Here he acquired a ranch and a wife, Mary Messmer. On September 14, 1875, one of McMains' friends, the Reverend Franklin J. Tolby, was murdered. McMains set out to find the killer, and during this search he became involved in the Maxwell Land Grant dispute. Spanish and Mexican land tenure was quite different from that of the United States. Those governments made large community and family grants. Each settler had his own individual plot, and there was a large area for the common use of all for grazing, hunting, and wood gathering. The Treaty of Guadalupe Hidalgo promised to recognize these grants in the territories obtained by the United States from Mexico, but the United States was slow to survey the land.

Lucien Maxwell had secured a Mexican grant of over 1,700,000 acres in New Mexico and Colorado territories. In 1870 Maxwell sold his grant but reserved 96,000 acres. By 1877 the company had surveyed about 1,800,000 acres, although many Americans had homesteaded the land believing it to be in the public domain. Railroads and other corporations had also made large claims.

McMains and the settlers claimed that only 96,000 acres of the claim were valid. In the Tamelin Supreme Court case the Court held that, in spite of the invalid Mexican claim, since the United States Congress had approved the survey and patent, it was now a claim *de novo* and was therefore legal. When Congress approved the Maxwell survey, McMains and all other settlers had either to come to terms with the company, whose tactics were not always fair, or to fight. McMains became the leader of several hundred settlers who chose to fight.

In the long fight against the Maxwell Land Grant, McMains made fourteen trips to Washington, one of which trips included an interview with President Cleveland in 1886. During the fight he served in the New Mexico Territorial Legislature, was tried and acquitted of manslaughter, but was

convicted of conspiracy and served six months in prison.

The long fight ended in failure when, on April 18, 1887, the U.S. Supreme Court ruled that the land was not in the public domain and the settlers had no rights superior to those of the original grantee. This was not just a struggle over land ownership but a battle against greed, injustice, and corruption. In 1894 McMains was defeated as a Populist candidate for the Colorado General Assembly. He died on April 15, 1899.

The original McMains papers were destroyed but not before the late Morris F. Taylor had made copies. Taylor used many primary sources including interviews with participants, church records, Land Office records, newspapers, pamphlets, and scores of secondary materials, all of which are documented in the work. The book also has a good index.

THOMAS LLOYD MILLER
Texas A & M University

LAWRENCE H. LARSEN. *The Urban West at the End of the Frontier*. Lawrence: Regents Press of Kansas. 1978. Pp. xiii, 173. \$12.50.

Until recently historians have neglected the examination of urbanization as a unifying theme in the history of the far western American frontier. In the last two decades, they have, however, produced a number of specialized local and regional studies of western cities and western urban topics. Lawrence H. Larsen skillfully draws on these studies, old local histories, and extensive primary material to present a fascinating description of cities and urban life west of the 95th meridian in the period primarily from 1880 to 1890. The frontier, of course, was officially pronounced at an end in 1890, and the period he chose permitted use of one of the most remarkable of our public documents, the unique "Report on the Social Statistics of Cities," which was a part of the 1880 census. This massive compilation of individual accounts of 222 American cities is Larsen's main source, and no one has used it to better purpose.

Larsen synthesizes and elaborates a number of themes about western urban development suggested by recent urban historians: the importance of urban promotion and rivalry in the location and growth of American western cities, the ethnic diversity of urban populations, and the emergence of a system of cities centered around the regional metropolises of Kansas City, San Francisco, and Denver. But the central thesis he develops is that western urban life was not distinctive, that western cities were calculatedly modeled on those to the east. The frontier did not reshape urban institutions; the mis-

takes and accomplishments of earlier urban society were repeated time and again. "The cities of the West," he emphasizes, "represented an extension of a process older than the Republic—exploration, settlement, and growth, a process that resulted in a nation of cities" (p. 121). Larsen proves his point, but his debate throughout the book with Frederick Jackson Turner over the innovative effects of the frontier may have already been won by others. His framework, however, does enable him to find a leitmotif in the vision of William Gilpin, a western promoter of Kansas City and Denver who offered a Turnerian and yet explicitly urban conception of the American frontier.

Although Larsen's examination of general themes is clear and sound, the strength of his book lies in his concrete descriptions of urban activities and institutions in the twenty-four western cities above eight thousand in population. He refreshingly avoids the mathematical examinations of mobility and related questions that are a part of an occasionally trite "new urban history" and is at his best when he writes of such down-to-earth matters as buildings, street cars, and garbage. Although his work is brief, it presents the same type of rich detail to be found in the works of Richard C. Wade and Carl Bridenbaugh on other urban frontiers in American history. Examining themes about western cities to be found in movies and television, he shows, for example, how little the "bucket brigade," "the young physician who saves the town," and of course the "brave, bold lawman," reflect the reality of fire protection, public health, or law enforcement in the western town and city. Larsen's work is more limited than Wade's or Bridenbaugh's and is not as original. But he makes a similar contribution. His sound and lucid book demonstrates that the frontier urban process to be found in Bridenbaugh's colonial cities and in those of Wade's Midwest persisted in the West of the late nineteenth century.

CHARLES N. GLAAB
University of Toledo

JOHN W. BAILEY. *Pacifying the Plains: General Alfred Terry and the Decline of the Sioux, 1866–1890*. (Contributions in Military History, number 17.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 236. \$18.95.

Alfred H. Terry is known to history as the scholarly lawyer-general who in the Civil War took Fort Fisher, North Carolina, after Ben Butler had failed and then, in the postwar years, commanded the Department of Dakota during the most intense hostilities with the Sioux and Cheyennes. Terry served on the commission that negotiated the Fort Laramie treaty of 1868 and is best known as George A. Cus-

ter's commander in the ill-fated Sioux campaign of 1876.

John W. Bailey has done a workmanlike job of chronicling Terry's career and assessing his significance. The portrait that emerges confirms previous images of a quietly competent and unusually thoughtful soldier, lacking the contentiousness and flamboyance of many of his contemporaries.

Beyond the usual official records, Bailey has used the Terry Papers at Yale University. Unfortunately they do not contain the riches to be expected from so literate a figure and thus do not permit significant new insights into either the personal or official dimensions of Terry.

Bailey sees Terry as a decisive voice in the deliberations of the Peace Commission of 1868, although he may have been as much simply the one who expressed the thoughts of Sherman, Augur, Harney, and other of the military members. Also, Terry took a much softer, more compassionate line on the Black Hills issue than Sherman, Sheridan, and Crook, believing that troops should be used to keep the miners out of this gold-rich region as long as the Indians owned it. This view, of course, did not prevail, and it does not appear that Terry urged it very persistently. In treating the Little Bighorn controversies, Bailey judges Custer harshly, much more harshly than did Terry himself. On this and other topics, Bailey would have profited from a deeper reading of secondary sources. Especially important would have been John Gray's recent *Centennial Campaign*, which has much to say about Terry.

Bailey's volume is a competent biography of a figure who has long deserved one and a useful addition to the literature of the Indian wars.

ROBERT M. UTLEY

Advisory Council on Historic Preservation

PAUL KLEPPNER. *The Third Electoral System, 1853-1892: Parties, Voters, and Political Cultures*. Chapel Hill: University of North Carolina Press. 1979. Pp. xxii, 424. \$21.00.

The Third Electoral System is the most important voting-behavior study to appear to date. In it, Paul Kleppner builds upon, but goes far beyond, his first full-length work, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850-1900* (1970). That pioneering monograph analyzed group voting behavior in three states before and during the electoral-political realignment of the 1890s. The present study treats all sections, particularly the Northeast, Midwest, and Border, from the realignment of the 1850s that gave birth to the Third Electoral System, through the Democratic upsurge of 1889-92 in the

Northeast and Midwest that revealed the continued potency of long-standing cultural issues in those areas and the Populist upsurge of 1890-92 in the West that anticipated the sectional-economic cleavages of the national realignment of the 1890s.

Kleppner makes clear his commitment to historical analysis as a means of developing social theory. Primarily interested in studying "the social bases of mass partisan support," he deals with party officeholders and workers only as they relate to the party-in-the-electorate. He contends that important sources of differences between the political parties lay outside of the parties themselves in antagonisms among social groups. Kleppner analyzes election and population data from counties and minor civil divisions in many states to establish the voting behavior of social groups, and a variety of primary sources to illuminate social-group conflicts and to explain why some became politically relevant in particular contexts and others did not. Kleppner argues, as he did in *The Cross of Culture*, that within the nonsouthern electorate, the primary conflict of political significance pitted evangelical-pietistic against ritualistic religious groups. However, his growing understanding of social groups in the Midwest and elsewhere has led him to refine his formulation of the pietistic-ritualistic continuum and to take into fuller account historical and contextual factors—for example, ethnocultural conflicts, status-group distinctions, political-generational experiences, economic conflicts and conditions—in explaining group voting behavior. That explanation is sophisticated and generally persuasive.

Approving the body of Kleppner's analysis does not necessitate accepting all its parts. The discussion of the electoral realignment of the 1850s does not make clear the relative importance of voter conversion and recruitment. Kleppner probably exaggerates the plight of the northern Democracy at the end of the Civil War. He offers scant evidence of an "accelerating" wartime Republican attack on Catholics. (More to the point in his treatment of "a revived Catholic question" during the 1870s.) Kleppner deals with many, but not all, of the issues raised by Melvyn Hammarberg's *The Indiana Voter* (1977). One wonders why Yankee counties shifted with, not against, German units during the Democratic revival of 1889-92.

Kleppner writes clearly, though he is repetitive and sometimes given to jargon (for example, "impacted," "repercentagized"). The University of North Carolina Press has served all well by providing footnotes, tables, and maps. In short, this book is a model of research, analysis, and presentation. It should influence all subsequent work in the field.

SAMUEL T. MCSEVENEY
Vanderbilt University

PAUL BOYER. *Urban Masses and Moral Order in America, 1820-1920*. Cambridge, Mass.: Harvard University Press. 1978. Pp. xvi, 387. \$18.50.

In the preface to this rich, widely ranging book, Paul Boyer recalls his boyhood in Dayton, Ohio, where he attended Sunday school at an urban mission founded by his grandfather, learned to swim at the local YMCA, and spent free time at a supervised playground run by the city. Now, after excellent studies of book censorship and of the repression of the Salem witches, he has produced a panoramic account of how the kinds of moral uplift and civic betterment that shaped his own early experience came into being. As in his previous books, Boyer adopts a social-control perspective, in which fears of disorder and of lower classes stand out as motives for doing good. This approach, while limiting sharply *what* he examines, is executed with subtlety and flexibility. The result is an insightful, important, multi-faceted history of reform.

Boyer perceives the modern city as a place of anonymity and diversity, in which Protestant reformers, from Lyman Beecher and the Tappan brothers onward, tried to recreate the cohesion and rectitude they associated with the small town. Their principal achievements in the Jacksonian era were tract societies and Sunday schools. Boyer gives us a fascinating analysis of how the tracts deliberately played on their readers' memories of an intimate village community, while the Sunday schools instilled orderly, deferential patterns of behavior. This section of the book shows how young tract volunteers and Sunday school teachers derived from their pious labors a comforting discipline and sense of fraternity among themselves.

Following the depression of 1837, increasing disorder among the urban poor, plus the setbacks that temperance reformers and Sunday school promoters met, led to more specifically focused enterprises: the Association for Improving the Condition of the Poor, the Children's Aid Society, and the YMCA. These mid-century initiatives were followed by the Salvation Army, the charity organization movement, and in the 1890s the political crusades of Charles Parkhurst and others. Each of these endeavors is sensitively described; but not until we reach the Progressive Era, to which the last third of the book is devoted, do the basic strategies of urban moral reform become altogether clear.

According to Boyer, two styles of reform, which earlier had intermingled, became sharply differentiated in the early twentieth century. A negative and coercive style, typified by efforts to outlaw the brothel and the saloon, concentrated on eradicating evil institutions. Another type of reformer, whom Boyer labels "positive environmentalist," repu-

diated repression in favor of environmental changes that would call forth more spontaneously the good behavior and the social harmony that all desired. This was the spirit of some housing reform (though tenement laws seem to belong to the first category more than the second), campaigns for public parks and supervised playgrounds, and above all the City Beautiful movement. Boyer is especially good in showing how civic beautification and the subsequent rise of city planning sprang from the belief that a physically attractive urban environment would inspire in the population at large a new upsurge of civic loyalty. Esthetic harmony would result in moral order. These environmental strategies, however, allowed reformers to stand aloof from the urban masses. By operating on the people indirectly, do-gooders gave up the personal contact their nineteenth-century predecessors had characteristically sought.

A final chapter on the 1920s celebrates a decline of the traditional American hostility toward the city and a concomitant fading of the old Protestant impulse to impose a single morality on a heterogeneous society. With the acceptance of urban diversity by American intellectuals the book comes to a happy ending. My own emotions were somewhat more mixed. I had learned a great deal about a variety of reform movements in an urban context, as understood in the light of a traditional social ideal their promoters shared. On the other hand, the fearful and backward-looking attitudes examined in these pages did not seem to me to embrace enough of "America's moral response to the city" (p. vii) to fulfill the larger aim of the book. Boyer's story is one of continuity and uniformity; but the ultimate challenge of his subject would seem to be an understanding of the ways in which and the extent to which the city has diversified our moral universe.

JOHN HIGHAM
Johns Hopkins University

DAVID R. JOHNSON. *Policing the Urban Underworld: The Impact of Crime on the Development of the American Police, 1800-1887*. Philadelphia: Temple University Press. 1979. Pp. viii, 240. \$15.00.

By describing three categories of criminal behavior in the nineteenth century and certain practices of early-nineteenth-century policing, David R. Johnson has tried to show how criminals determined police behavior. His thesis is that police, "supposedly controlled" by "public and administrators," were in fact at the mercy of changing patterns of crime. But the book contains virtually no evidence to support this interesting thesis, except in its description of detectives' corrupt relationships with vice and certain

theft operations. It is not surprising that his thesis cannot be supported, because very little of nineteenth-century policing was concerned with crime control. Most police work had to do with non-crime-related services; and professional criminals and their counterparts in the police departments, detectives, while media sensations, simply never made up much of the sum of the activity of the urban police. While it may be fair to say that the goals that police administrators and, perhaps, the public set for the police were never attained, the reasons for this failure range from faulty theory to flawed implementation. Including criminal behavior in an account of nineteenth-century policing can be instructive, but it cannot hope to provide either necessary or sufficient conditions to explain the behavior of urban police.

Certainly this book's major contribution to social history has nothing to do with its thesis. Rather, its value is as a description of certain kinds of crime, mainly in Chicago and Philadelphia, with occasional material on New York or other cities. Basing his work primarily on newspaper sources, Johnson has also pulled together frustratingly scattered descriptive material on juvenile gangs, the methods of professional thieves, and the evolution of urban gambling and prostitution in the first three-fourths of the nineteenth century. Particularly in the area of gambling, he has described the growth and operation of syndicates, the emergence of faro and policy, and the changing forms of betting on horse races. These areas of social behavior, for obvious reasons, had a good deal of secrecy surrounding them, and by pulling together material from newspapers, buttressed by memoirs and earlier histories, Johnson seems to have written a credible description of these kinds of criminal behaviors, particularly vice.

The question of sources, however, becomes of great concern when describing such things as the first gambling syndicate (C. H. Murray and Company, New York, 1861). Johnson too eagerly accepts newspaper accounts and self-serving memoirs at face value. Given that such sources are the only sources, one must be cautious in accepting his or nineteenth-century observers' estimates of the numbers of prostitutes, brothels, or the annual cash intake of gamblers. At some points Johnson's uncritical use of sources leads to inaccurate descriptions. For instance, he asserts that police officers kept arrest tallies to justify promotions, citing sources for New York and Chicago. Yet of the two Chicago sources, only one mentions arrests per officer. Criticizing some officers for arresting too many people (thirty in a month) says nothing about arrests and promotions (pp. 129, 219). Similarly, the book abounds with unsupported factual assertions, many of which should have been important questions for

empirical research: for example, urban rioting declined after 1860; assaults on police peaked in the early 1850s; arrests on "suspicion" caught "numerous" thieves; and police collusion with vice entrepreneurs began in the 1840s (pp. 134, 136, 131, 170).

Possibly the best parts of Johnson's book are the remarks and observations scattered throughout on the notion of crime prevention. Here he discusses how the prevention idea implied an ability to predict (p. 19) and points out that for police to achieve the semblance of successful crime prevention they had to opt for "criminals' cooperation to achieve the appearance of law enforcement" (p. 185). Simply by emphasizing how little today's law enforcement techniques have changed from those of the nineteenth century, Johnson demonstrates that the social complexity of criminal behavior will not admit easy deterrence solutions. This perspective is one that many criminologists have come to recently. That a historian, working with recalcitrant nineteenth-century materials, can come to the same position bodes well for the relevance of historical scholarship.

ERIC H. MONKKONEN
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JOHN T. CUMBLER. *Working-Class Community in Industrial America: Work, Leisure, and Struggle in Two Industrial Cities, 1880-1930*. (Contributions in Labor History, number 8.) Westport, Conn.: Greenwood Press. 1979. Pp. xiv, 283. \$22.95.

Labor historians have long known the important differences between textile workers and those employed in shoe factories. In this convincing comparative study of laborers in Lynn and Fall River, John T. Cumbler points to similarities as well. In both Massachusetts cities, Cumbler finds nineteenth-century working-class communities that integrated newcomers, provided fellowship, offered support in adversity, and nurtured solidarity against employers. For different reasons, these two communities became less cohesive in the twentieth century. But factories were not to blame for individual isolation and alienation, as some critics have argued. Rather factories provided the web of human relationships that sustained people in these cities.

Social institutions in Lynn and Fall River absorbed newly arrived French-Canadians, some of whom had come as strikebreakers. Through contact at work, in boardinghouses and taverns, at lodges and clubs, at dances, parades, and rallies, these immigrants learned to identify with others in the working class. In many cases, in spite of opposition from their priests, French-Canadians joined unions

and picket lines. Especially in Fall River, the strikes were usually broken. But Cumbler demonstrates that the working-class unity that helped sustain strikes endured until economic and demographic changes in the twentieth century disrupted it. Then workers in Fall River, fearing for their jobs and for the future of their industry, did not accept Polish and Portuguese newcomers. The shift in Lynn from the production of shoes to electrical equipment modified social institutions and habits, partly because the General Electric plant was geographically remote from the central part of the city where shoe workers had lived and interacted. "Consumerism," Cumbler asserts, and the attempt to secure personal economic security replaced the working-class community that once characterized both cities.

This book bears the stamp of the "new" labor history, now about two decades old. In these studies, trade unions, labor leaders, and reform politics—once the central concerns of labor historians—take the subordinate place they occupied in the lives of workers themselves. The scholarship focuses on specific local data; Cumbler's is the third recent work on laborers in Lynn. But the imaginative use of these data—including photographs in this book, which are in fact visual evidence and not merely diverting illustrations—is broadening our understanding of American social history.

The book has defects. Cumbler needed more editorial assistance than he received, and his publisher badly needs a new proofreader. The index is perfunctory. The explanation for the failure of Fall River workers to welcome Polish and Portuguese immigrants is not entirely persuasive, and "consumerism" is not a rigorous analytical concept. These shortcomings, however, do not diminish the central part of Cumbler's study in any fundamental way.

HENRY F. BEDFORD
Phillips Exeter Academy

CARLOS A. SCHWANTES. *Radical Heritage: Labor, Socialism, and Reform in Washington and British Columbia, 1885-1917*. (Emil and Kathleen Sick Lecture-Book Series in Western History and Biography.) Seattle: University of Washington Press. 1979. Pp. xviii, 288. \$14.95.

The Pacific Northwest, long judged a center of radical socioeconomic experimentation, has frequently appalled middle-of-the-road American and Canadian reformers, and one exasperated New Dealer even assertedly characterized Washington state as America's "Soviet." Carlos A. Schwantes is the first to tap a rich lode of archival material as well as thesis and dissertation literature to describe the historical origins of that tolerance of radical activism

and to account for the traditions and other particularisms that have sustained a "radical heritage" in the Northwest, particularly in British Columbia and Washington. There, residents were heavily late-nineteenth-century newcomers who, in searching for a better social order, joined world-savers in testing virtually all reform proposals generated during the Gilded Age. Eventually the ideas of labor reformers, socialists, and agrarian radicals won the greatest esteem.

Since lumbering, mining, and commerce sustained the Northwest economically, labor reformers there cultivated a fertile vineyard in which plentiful and conflicting labor union movements proliferated. *Radical Heritage*, therefore, is primarily an account of those movements. The narrative revolves around Schwantes's thesis that unionists, their farmer allies, and some Socialists fashioned a distinctive "regional unionism," which unionists maintained in the twentieth century, although the American Federation of Labor eventually absorbed many British Columbia and Washington unions. Unable to obliterate the Northwest unionists' unique beliefs and practices, which conflicted sharply with AFL orthodoxies, hapless AFL hierarchs and their Northwest emissaries could only grimace. Washington unionists persisted in tugging in union with agrarian rebels, and both British Columbia and Washington AFL members saw no heresy in their industrial union philosophies. Likewise, British Columbia unionists discounted pronouncements from AFL President Samuel Gompers that socialism was "silly" and pioneered labor-socialist political action.

Other activists contributed measurably but secondarily to establishing the Northwest's "radical heritage." Among them were agrarian radicals, assorted utopians with communal experiments on their minds, and middle-class Progressives who, doubtless in great mortification, share a chapter in *Radical Heritage* with Wobblies. More importantly, by Schwantes's reckoning enigmatic and eternally disputatious socialists supplied ideas and creative leadership for the enduring and sometimes unabashedly radical quest of Northwest residents for a more equitable society. But for reasons that the author might have made clearer, British Columbia socialists and agrarian socialists in Washington preempted socialism's laurels in the Northwest and exercised vastly greater influence than did the crowd of urban socialists in Washington.

Probably some readers will deny that Northwest unionism was so distinctive; others may insist that nonlabor groups bear more responsibility for Northwest radicalism. Nevertheless, this book offers plenty that is worthwhile. Thanks to Schwantes's expertise on laboring men and their organizations, we now have a reliable history of pre-1917 North-

west unionism. His lively chapters on Northwest socialists contain new material and invite additional research. Schwantes's sketches of agrarian radicals open the door to further study of Northwest farmer-labor movements.

HUGH T. LOVIN
Boise State University

WALTER F. PETERSON. *An Industrial Heritage: Allis-Chalmers Corporation*. Milwaukee: Milwaukee County Historical Society. 1978. Pp. 448. \$17.50.

Allis-Chalmers, now a large manufacturer, started out modestly in 1861. That year, Edward P. Allis, a New Yorker who had moved to Milwaukee, bought the Reliance Works, manufacturers of milling, sawmills, and cast iron products. Allis had also purchased in Milwaukee twenty acres on which he later moved the Reliance Works and built other factories. After several periods of fiscal difficulties and friction both internally and with organized labor, the corporation merged with three others, one of which was Fraser & Chalmers, and in 1901 formed the Allis-Chalmers Company. A power struggle was resolved with the appointment in 1912 of General Otto H. Falk. The company, a combination of divisions, had taken the form we know today. Allis-Chalmers has preserved its independence, relations with the labor unions are stable, and sales in 1976 reached a peak of \$1.5 billion.

Two figures predominated: Edward Allis and Otto Falk. They had a common pattern of operations: hard work, good equipment, accountability, able personnel, diversity, flexibility, and venturesomeness. More unusual was their emphasis on research, almost constant physical and product expansion, and divisional structure. The original divisions of sawmill, flour milling, and steam engines became virtually autonomous units in research, production, and sales. Falk's special achievement was adding the farm tractor division, which by 1938 was the major source of company profit.

Both authors are familiar with engines and with Allis-Chalmers. Walter Peterson, now President of University of Dubuque, provides ten carefully organized, clearly written chapters bringing the story to 1965. Although there is no mention of the connection anywhere in the volume, Peterson's role as consultant to the company since 1959 might have provided special insights. It is unfortunate that either he or his publishers has chosen to be silent on this connection, raising questions where clarification might have provided even greater authority for his very full treatment. Edward Weber, a dean of business administration, prepared the epilogue which brings the account down to 1977. The most exciting sections describe how the tractor division

turned defeat into success and how the company fought efforts by conglomerates to take over. The bibliography is fuller and more varied until 1945; thereafter it depends heavily on the annual reports of the company. Although the tone of the main part of the book seems to lack sufficient criticism, the work is certainly a useful addition, for more histories of companies are badly needed.

WILLIAM CHAZANOF
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Fredonia

ROBERT EMMETT CURRAN. *Michael Augustine Corrigan and the Shaping of Conservative Catholicism in America, 1878-1902*. (The American Catholic Tradition.) New York: Arno Press. 1978. Pp. xiii, 547. \$34.00.

In another of its remarkable contributions to the literature of American history, Arno Press has produced a series of volumes on American Catholicism, including Robert Emmett Curran's fine Yale doctoral dissertation on New York's Archbishop Michael Augustine Corrigan. Scholars like Gerald Fogarty, Thomas Wangler, and Margaret Reher have been rewriting the history of American Catholic liberalism in the late nineteenth century, and Curran has now examined the major conservative spokesman of the period. Students of the Americanist movement have found American Catholic liberalism to be intellectually serious and more politically organized than was thought to have been the case. Curran finds that the conservative side, represented by Corrigan, had considerably less intellectual sophistication than their liberal adversaries, but at least equal political skills.

In the aftermath of the Third Plenary Council of Baltimore in 1884, serious divisions developed in the American hierarchy. Liberals like John Ireland, Denis O'Connell, and John Keane questioned the need for a highly organized Catholic subculture, pushing instead for the rapid assimilation of immigrants and creative engagement with secular culture. Hopeful and optimistic about the church's future in a democratic and progressive era, they were Americanists both in theory and practice. Their ideas, and by implication their policies, were eventually rejected by Rome, a defeat that in part resulted from the deliberate combat waged against them by Corrigan. Rome was the real arena of the struggle, and influence with the new Apostolic Delegate was the key to success. Initially, the liberals enjoyed favor with the curia and influence with the delegate; but Corrigan formed his own alliances with curial factions on the basis of contacts made during his student days. In the end, his theological conservatism, his concern for diocesan autonomy and episcopal control, and his pastoral priorities

corresponded far more closely to the goals of the ultramontane papacy than did the venturesome Americanism of his opponents. Corrigan's victory meant, according to Curran, the formation of a rigid Catholic subculture that "abandoned any attempt to find an equation between Catholicity and Americanism."

Curran is strongest in relating Corrigan's policies to his personality. Sheltered as a youth in a prosperous family, lacking pastoral experience, and experiencing ecclesiastical advancement because of his Roman connections, Corrigan was an unusually insecure bishop. In New York he confronted a well-organized group of very popular and aggressive priests headed by Edward McGlynn. His clash with McGlynn over the latter's political activism led to McGlynn's excommunication, followed by a long series of actions designed to embarrass and humiliate Corrigan's clerical opponents and force his priests to accept his domination of diocesan affairs. According to Curran, Corrigan "was almost pathologically prone to a fear of ubiquitous conspiracies" and not at all reluctant to use deception, spying, loyalty oaths, and preferential assignments to secure his authority and power. Although Corrigan's personality played an important role, one suspects that his victory over the Americanists reflected the requirements of a maturing American Church confronted with massive waves of new immigrants and the needs of a triumphant Roman Ultramontan-ism. As a case study of the politics of the church and the personality of an individual bishop, Curran's candid, thorough essay can hardly be matched in the historical literature of American Catholicism.

DAVID J. O'BRIEN
Holy Cross College

MICHAEL N. DOBKOWSKI. *The Tarnished Dream: The Basis of American Anti-Semitism*. (Contributions in American History, number 81.) Westport, Conn.: Greenwood Press. 1979. Pp. x, 291. \$22.50.

The Tarnished Dream is a striking book. Not since the 1950s have American historians engaged in serious analysis of the roots of anti-Semitism in the United States. Now Michael N. Dobkowski, assistant professor of religious studies at Hobart and William Smith Colleges, once again explores the subject more broadly and deeply than Handlin, Hofstadter, and Higham did a generation ago. Like them he focuses primarily on the Gilded Age and Progressive Era, but unlike his predecessors he demonstrates the pervasiveness of anti-Semitism in American society. Demolishing as "a misleading oversimplification" the myth that "overt and articulate anti-Semitism . . . rested primarily with the anti-Semitic rhetoric of the Populist movement" (p.

174), Dobkowski argues that Handlin, Hofstadter, and Higham displayed "an inclination to deny the anti-Jewish character of the most obvious and flagrant stereotypical expressions that appeared frequently in literature, on the stage, and in print" (p. 6).

One can certainly not accuse Dobkowski, however, of this transgression. His extensive discussions and bibliography testify not only to the thoroughness of his reading but also to the ubiquitousness of anti-Semitism in the United States. To reach for an understanding of this complex phenomenon, Dobkowski analyzes, in single chapters, religious origins, images of the Jew as criminal and lover of gold, patrician sourness, the impact of the immigrant influx, and the attitudes toward the radical Jew.

What Dobowski has done in his treatment of anti-Semitism during the fifty-year period between the 1870s and 1920s should be a model for historians examining anti-Semitism in other eras of the American past. To date, sociologists and psychologists have produced the most insightful works on the subject (for example, T. S. Adorno, Charles Y. Glock, Gertrude Selznick, Rodney Stark, and Stephen Steinberg). We know too little about anti-Semitism in colonial America and the young republic. Isolated snippets about *Niles Register* and the oft-repeated order by General Grant ridding his military zone of Jews during the Civil War do not provide sufficient data for historical scrutiny. But careful study of literary, journalistic, autobiographical, and other sources should shed light on anti-Semitism in the United States before the 1870s.

I cannot conclude this review without re-emphasizing the importance of Dobkowski's contribution. He has not only proven the existence of a deeply rooted, widespread anti-Semitism in the United States in the Gilded Age and after, but also his accomplishment immediately ranks him with the best and most promising younger scholars of American Jewish history: Leon Jick, Steven Hertzberg, and Hasia R. Diner.

LEONARD DINNERSTEIN
University of Arizona

TIMOTHY P. WEBER. *Living in the Shadow of the Second Coming: American Premillennialism, 1875-1925*. New York: Oxford University Press. 1979. Pp. vi, 232. \$14.95.

Until fairly recently American conservative Protestantism in the post-Civil War era had been terra incognita to most historians. The stiff-necked traditionalism of many churchgoers in that epoch seemed far less worthy of attention than the innovative responses of social gospelers and theological liberals to the challenges of a rapidly developing

industrial society. The evident anti-intellectualism of those committed to revivalism, the holiness movement, or pentecostalism also probably caused historians of American religion to turn to other, more congenial, topics. Over the past decade or so, however, detailed monographic studies of scholars like Ernest Sandeen, Timothy Smith, and William McLoughlin began to reveal in detail the historical lineaments of late-nineteenth- and early-twentieth-century conservative Protestantism. Now the process of recovery of this tradition may be advanced a step further as contemporary conservative churches, experiencing a new respectability in the public eye, spawn their own group of young scholars determined to explore essential elements of a common past.

The book under review, by Timothy P. Weber, a young church historian at Conservative Baptist Theological Seminary in Denver, seems to fit such a mold. Weber's essay is a reworked Ph.D. dissertation from the University of Chicago (anathema to conservative Protestants just a generation ago). The book explores in detail premillennialism—one of the key intellectual-theological movements that shaped post-Civil War Protestantism. Weber is interested primarily in explaining how premillennialists of the late nineteenth and early twentieth centuries lived out the peculiarities of their faith, how "beliefs were translated into daily life" (p. 8). The author examines the premillennialists' distinctive emphases upon eschatology and the hovering imminence of a cataclysmic second coming of Christ and the effect of such beliefs on personal approaches to evangelism, to social activism and reform, and to the events of World War I. The early sections of the book are interesting, but they also include many repetitious passages and too often lack historical specificity.

Weber's analysis becomes more effective as the narrative proceeds. He includes a fascinating chapter analyzing the premillennialists' special (sympathetic) views toward Jews and the early Zionist movement. He also includes a detailed discussion of the premillennialists' role in the fundamentalist-modernist controversy of the 1920s; his thoughtful comments add considerably to our understanding of fundamentalism, a movement that continues to attract scholarly attention.

The author's exploration of the origins of post-Civil War premillennialism is conventional and too brief. He needs to explore much more thoroughly the powerful millennial strain in Calvinist thought in the colonial and early national eras and to seek connections between these earlier theological tendencies and the attitudes of premillennialists of a later time. Weber's use of terminology also at times is confusing, especially when he essentially equates dispensationalism—a special variant in the move-

ment he studies—with premillennialism yet also discusses nondispensational premillennialist groups. Although the book has flaws, it remains an important contribution to the steadily growing literature on conservative American Protestantism since the Civil War. It is also a testimonial that the contemporary conservative Protestant community, long regarded with suspicion by professional scholars, is ready to offer its own serious contributions to efforts at reconstruction of the past.

JAMES F. FINDLAY

University of Rhode Island

ROBERT C. TWOMBLY. *Frank Lloyd Wright: His Life and His Architecture*. New York: John Wiley and Sons. 1979. Pp. x, 444. \$19.95.

Frank Lloyd Wright, who passed away twenty years ago, was one of this nation's—and the world's—most important practitioners and theoreticians of environmental planning and design, firmly implanted as much in the popular mind as in scholarship. As Robert C. Twombly amply shows, Wright was one of those larger-than-life figures whose work and thought continue to speak to us with clarity and feeling. The various currents of Wright's career originated in the nineteenth century and crested at consistently more creative levels in the twentieth. They were fed by hopes and fears concerning Western civilization as evident today as a half-century ago.

Wright's achievement was to evolve out of his own genius, personal experience, and wide reading in the seminal thought of his age—for example, Henry George, Thorstein Veblen—a "radical" interpretation of society. Simultaneously, he formulated a "modern" understanding of environmental forms—the house, community, city, region—incorporating this thought. Most distinctive, of course, was the design quality of his built structures. These were as technologically innovative as they were functionally and visually satisfying to users and viewers. He left a legacy in concept and artifact that will continue to be important so long as the issues that concerned him remain to challenge us—such as unplanned urbanization, a declining environmental quality, wasteful use of energy, need for sound and attractive low-cost housing, and international cooperation based on regionalism.

Among a large and growing literature, Robert Twombly's biography remains the most complete one-volume description of the person and his work. (This is a substantial revision of Twombly's earlier study, *Frank Lloyd Wright: An Interpretive Biography* [1973].) In fourteen dense, well-written, and illustrated chapters, organized chronologically and topi-

cally, the author consistently holds the reader's attention even as he manages to keep in fairly equal balance several different themes explaining Wright's life and work.

What we are offered in addition to an account of a life replete with personal tragedy and enormous professional success—early experienced—is a very perceptive analysis, based on exhaustive documentation, of different aspects of Wright's career. The analysis includes the psychological motivation of Wright's work, the historical forces that were reshaping such basic institutions as the family, Wright's consistent efforts to devise for himself and others a usable social and urban theory, and a sensitive description of the design composition of Wright's architecture, always relating it to the other themes being considered.

Twombly is as interested in the process and technology of planning as in the design attributes of the product itself. The reader is informed as to who Wright's clients and patrons were, the various technological innovations he pioneered in heating and air conditioning, the use of new materials in construction, the truly "radical" and "modern" implications of the interior spaces he created in private residences and commercial office buildings, his remarkable plans for governmental centers such as the Marin County Civic Center, the use and shape of the land as an integral feature of the design, and the relationship between Wright and those many others involved in the creative and cooperative process of planning and design.

In sum, this is an important work about an unusually creative and courageous American who has given us so much to appreciate and to think about regarding the quality of this nation's environmental past and its future.

ALBERT FEIN
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RICHARD E. TITLOW. *Americans Import Merit: Origins of the United States Civil Service and the Influence of the British Model*. Washington: University Press of America. 1979. Pp. vii, 348. \$11.25.

Confucian teaching tells us that "the essence of righteous rule is moral guidance." In Western societies, as in China, civil service reform typically has sought to bring some semblance of moral authority to the everyday conduct of governmental affairs. By the time reformers in the United States were strong enough to press their demands with some reasonable chance of succeeding, the British civil service had progressed to a point where "righteous rule," if not assured, at least had routed the worst of the

spoilsmen and other civic predators. The Americans carefully examined the systems whereby several nations managed their governments; but they turned to the British, in the end, for practical guidance in developing the civil service system that the Pendleton Act authorized in 1883.

In this study of foreign influences on American civil service, Richard E. Titlow first discusses briefly the Chinese, Prussian, and French models, then offers a chapter survey of the British experience with civil service and reform at home and in the empire. There is nothing new in the recapitulation of American administrative history that follows, although it is useful in bringing the reader well informed to the main thrust of the book: an examination of the interaction between British and American reformers in the years after the Civil War. Titlow observes that the movement in the United States should be viewed in the context of Anglo-American developments, not exclusively as an American political phenomenon. Beginning with Charles Sumner and Thomas A. Jenckes, American reformers consciously patterned their efforts and proposals after the British models, and the influence continued into the actual administration of the system after passage of the Pendleton Act. Indeed, that landmark measure incorporated all the main elements of the British administrative system, and the American system today still strongly reflects the debt to the "Old World."

Americans Import Merit is a competent and comprehensive, if hardly inspired, administrative history, developed from an impressive array of resources on both sides of the Atlantic. In the end, however, the author's thesis regarding the interaction of British and American reformers continues to rest largely on inferences and common sense. The problem is not so much his as it is the dearth of specific materials to document the relationship (which he readily acknowledges), particularly personal correspondence containing specific interchanges of views. Doubtless, this lack also helps to explain another weakness of the book—a failure to interweave the two national experiences in such a way as to elaborate significantly upon Robert Kelley's delineation of an Anglo-American community in the nineteenth century. Titlow knows the community existed; his practice of treating its two elements separately does not bring it into clear perspective.

We have here a full and well-documented history of the origins of the American civil service movement. Had the author done some badly needed cutting and revising or had the book fallen into the hands of a good editor, we might also have a history to appeal to readers other than specialized students of the subject.

JOHN G. SPROAT
University of South Carolina

GLENN C. ALTSCHULER. *Andrew D. White—Educator, Historian, Diplomat*. Ithaca, N.Y.: Cornell University Press. 1979. Pp. 300. \$17.50.

Along with Eliot and Gilman and Harper and Washington, the name of Andrew Dickson White has ranked high among the educational entrepreneurs of the Gilded Age. His successor as president of Cornell University, Charles Kendall Adams, once wrote to him, "When we are all passed away the thoughtful scholar who visits these hills will pronounce blessings on your name." The judgments that Glenn C. Altschuler pronounces on White make a mockery of Adams's pious forecast. His book consistently diminishes the complexity of the challenges White confronted across his long and varied career. Above all, the creation from scratch of a large, high-quality, coeducational, nonsectarian public university in the cockpit of post-Civil War educational politics was an organizational chore of awesome subtlety for a man of White's genteel background and soaring ideals. It merits an assessment of equivalent subtlety. Hugh Hawkins' account of Eliot's similar (though in many ways less complicated) achievement at Harvard springs to mind. While Altschuler treats White's years in Ithaca with factual care, his interest in the man clearly lies elsewhere. Early on, he opens hostilities with White's nineteenth-century mind.

White shared his generation's faith in linear human progress and believed that education and trained reason were important means to this end. His biographer uses these convictions as the standard for a relentless critique of White's actual behavior as an educator and public servant. The result is an indictment of White's talent for compromising his own ideals in the interest of survival and personal fame. The portrait emerges of a self-serving, disingenuous administrator who shunned authentic female equality, championed didactic Christianity, was guilty of occasional intellectual dishonesty, and tolerated crimes against academic freedom. White earns even lower marks for his ministries in Berlin and St. Petersburg. His failure to perceive emerging conflicts of interest between the United States and the new Germany he so greatly admired, his inability to understand Russia, and his inept handling of specific controversies such as the Bering Sea seal dispute of 1892 and American-Germany rivalry in the Philippines in 1898 betray in Altschuler's eye a persistent capacity for naïve, myopic blundering.

Finally, Altschuler finds little to admire in White's personal character and social attitudes. His blatant elitism, racism, and shortcomings as a husband and father receive close attention. It is as historian that he fares best, but even here he is faulted for his moral absolutes and mental confusion. At

the heart of White's problems, we learn, were his deeply conservative vision of a good society, his faith in education as a panacea for its imperfections, and his stubborn rhetorical commitment to progress in the teeth of all evidence to the contrary. The onset of world war in White's last years provides the climactic refutation of his life-long hopes. Death and destruction clinch the analysis. History is what happens to White's presumptions, and he is flattened by the outcome.

White has waited a long time for an adequate biographer. Despite its caustic tone, Altschuler's effort is well documented, aggressively argued, and thematically consistent. It is far from a definitive "life," but one senses that its publication must be a very satisfying victory for the author over his subject.

GEOFFREY BLODGETT
Oberlin College

RICHARD W. FOX. *So Far Disordered in Mind: Insanity in California, 1870-1930*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xvi, 204.

In the recent revival of interest in social deviance and institutional solutions to human problems, flimsy generalizations about mental illness and mental institutions have been fashionable. The kind of work that Richard W. Fox has done in this fine monograph has been in short supply, which is why it is an important book. His conclusions, though informed by sophisticated awareness of social theories, rest on detailed, careful study of data; they reflect creative use of source materials. With the aim of delineating "the social, institutional, and professional network within which thousands of deviant individuals were officially declared insane" and of elucidating "the civil commitment process itself" (pp. 3-4), Fox analyses the commitment records of the insane for San Francisco City and County from 1906 to 1928. Although the scope of his study is broader, this material is its heart.

Working with a sample of the commitment records, Fox draws a social profile of persons considered insane. On the whole he handles with care the sometimes small portions of his sample that represent certain social attributes. Sometimes, however, his penchant for statistics gives an exaggerated accuracy to data that involve complex issues and problematic evidence, such as the percentages purporting to show how many people considered for commitment actually met the legal requirements for involuntary hospitalization. Fox's general conclusions, which are convincing, tend to corroborate known patterns of state hospital populations: there was a high concentration of working-class persons, the foreign born, and women, and the hospitals were dumping grounds for troublesome, indigent,

homeless people—the aged, the senile, and the mentally handicapped, as well as the “bona fide” insane. Compared with other states, California seems to have been exceptional in the high proportion of the total population that were committed to mental hospitals, a situation caused in part by the absence of alternative institutions that were common elsewhere.

Fox is critical of current revisionist approaches, most notably Thomas Szasz’s stance that mental illness is merely a social construct and David Rothman’s thesis that mental institutions served primarily as devices for social control. In studying the commitment process, Fox believes he is analyzing a real, not a pseudo-problem (as Szasz would have it), and he perceives mixed motives for commitment and multiple social roles for mental institutions. Yet he is himself not exempt from polemicizing. His work has an antipsychiatry bias that blinds him to the full significance of evidence indicating that psychiatrists were the only influential organized group opposing the use of state hospitals in California as human warehouses. After virtually making the same point, he attributes the effort of psychiatrists to gain control over admissions to self-interest, their desire to build more hospitals and provide more jobs for themselves. But could not their self-interest also encompass a need to exclude inappropriate patients from their overcrowded hospitals so that they could gain professional respect by acting like physicians and helping patients to recover rather than having to act like prison wardens? Fox himself shows that laymen and general medical practitioners forced the hospitals to accept the people they sent and thus made them repositories for all sorts of social deviants, against the wishes of hospital superintendents.

It is to Fox’s credit that one can thus argue with his interpretations. Notwithstanding his admiration for Michel Foucault and like-minded critics, Fox’s work stands apart from theirs, especially Foucault’s, in presenting the evidence upon which his conclusions are based and thereby enabling readers to draw their own conclusions. This is no small distinction today when so many writers produce ideological or political tracts disguised as research.

NORMAN DAIN
Rutgers University

NORMA FAIN PRATT: *Morris Hillquit: A Political History of an American Jewish Socialist*. (Contributions in Political Science, number 20.) Westport, Conn.: Greenwood Press. 1979. Pp. xi, 272. \$18.50.

Norma Fain Pratt’s *Morris Hillquit* narrows a major gap in the historiography of the American left, of-

fering the first full-length study of a man who was a veritable fixture in the socialist movement for some forty years. Hillquit, after playing a key role in the rebellion against DeLeonite dominance of the Socialist Labor Party in 1899, led his defeated SLP faction into union with the Social Democratic Party of Eugene Debs and Victor Berger to create the Socialist Party of America in 1901. And from the SPA’s founding until his death in 1933, he exercised enormous influence on party affairs, serving for many years as a pivotal member of the national executive, representing the SPA at international socialist gatherings, carrying the party standard into several election campaigns in New York, and engaging actively in propaganda work as a writer, speaker, and debater. Paralleling Hillquit’s political career was his busy legal practice, which saw him defending radicals in civil liberties cases and acting as counsel for fledgling unions in the turbulent garment industry.

Pratt relates Hillquit’s story in the mode of traditional political biography, focusing on Hillquit the public figure and never straying far from straight narration. Accordingly, there is little here that is novel or provocative. But she gives us a thoroughly researched account in clear, if somewhat flat, prose and does not shrink from critical judgments. While detailing Hillquit’s abilities and contributions, Pratt also reveals her subject’s manipulative and opportunistic tendencies, his penchant for the good life, and his pronounced elitism. She develops this negative side of her portrait particularly in her discussion of Hillquit’s paradoxical relationship with his political and professional base, New York’s working-class, Jewish immigrants. A product of the Yiddish-speaking milieu of the sweatshops, Hillquit was nevertheless an assimilationist with no faith in the viability of a distinctive *Yidishkeit* and a man of bourgeois sensibilities who could neither identify with nor completely trust rank-and-file workers.

Unfortunately, Pratt declines to plumb beneath the surface of such contradictions and render more of Hillquit the man. Her book would have been far better had she provided it with a thematic core that attempted to explain motivation and perhaps place Hillquit’s experience in some larger context. As it is, her treatment brings us no closer to a resolution of the knotty question of why socialism failed to establish itself as a vital force in American society, a question that Pratt states to be one of her study’s more general concerns. The pity of it is that a convincing motif was within the author’s grasp. Interpreting Hillquit’s behavior as the consequence of thwarted youthful ambitions to achieve middle-class status in his native Latvia, a subsequent striving for upward mobility in America, and a desire to preserve the rewards of that climb would have been suggestive and quite plausible.

Despite its shortcomings, however, this study is a welcome addition to the literature.

I. GLEN SERETAN
University of Western Ontario

CRYSTAL EASTMAN. *On Women and Revolution*. Edited by BLANCHE WIESEN COOK. New York: Oxford University Press. 1978. Pp. xiv, 388. Paper \$4.95.

WILLIAM L. O'NEILL. *The Last Romantic: A Life of Max Eastman*. New York: Oxford University Press. 1978. Pp. xix, 339. \$14.95.

For historians who would use biographies not as chronicles recording the lives of "great men" but as vehicles illuminating important facets of the American experience, Max and Crystal Eastman are apt subjects. The offspring of two Congregational ministers, both son and daughter were strikingly handsome, extraordinarily vital individuals whose personal and intellectual lives—Max's especially—intersected some of the most significant political movements of this century. Activists both, they helped in pre-World War I years to forge that union of political radicalism and personal (and sexual) liberation so characteristic of the cultural rebellion that flourished in Greenwich Village and elsewhere. After the war, Crystal, who, as an attorney, had pioneered in the field of labor legislation and industrial safety, continued her career as a journalist and crusader, working tirelessly on behalf of feminism, socialism, peace, and civil liberties until her death in 1928 at the age of forty-six. Her articles and essays from these years have been perceptively edited by Blanche Wiesen Cook, who intends a biography when all family papers are available. Even these brief journalistic pieces convey a militance sustained by a tough-minded intelligence that gives the writings on feminism especially a contemporary relevance quite apart from their utility as historical documents, for Crystal Eastman knew that a feminist revolution, like a socialist revolution, was, in Cook's words, "a process not an event." Although scholars of the period have been aware of the broad parameters of Crystal's activism, we are indebted to Cook and to Oxford for making so readily accessible writings that will better enable us to evaluate her significance.

Max's career was longer; he died in 1969. It was also more complicated by virtue of its ideological span, for Eastman, although consistent in his vocations as journalist, poet, and libertarian, moved from the joyous radicalism of the prewar *Masses* to unabashed Trotskyism in the twenties, a "premature" anti-Stalinism in the thirties, and finally in the forties and fifties to a vigorous anticommunism and defense of free enterprise espoused from the

pages of the *National Review* and *Reader's Digest*. This intellectual journey coupled with a richly textured life—it was Max Eastman who smuggled Lenin's Testament out of Russia—invite a first-rate biography. Such a study would combine a compelling portrait of a compelling individual with a penetrating understanding of a mind grappling with important intellectual and cultural concerns. (The relationship between political democracy and socialism and the use—or misuse—of art as a political weapon were but two of the many problems that aroused Eastman's intellectual passion.) O'Neill's study, despite its strengths, disappoints on both counts. As biographer, he provides us with a brief foray into psychohistory. At issue are Eastman's intense linkage to an extraordinary mother and sister, his inability to form "mature" emotional attachments to the many other women who filled his life, and his search for father figures in Dewey and Trotsky—father figures whom Eastman, according to his biographer, always felt compelled to "overthrow . . . with his pen." But speculations about an unresolved Oedipal crisis do not substitute for the painstakingly crafted portrait of the secret self that Leon Edel argues is the art of biography.

Given the fullness of Eastman's own memoirs, it is perhaps understandable why O'Neill chose to concentrate on the writings and Eastman's relation "to what might be called the social history of ideas." Here, too, the results are mixed. Given Eastman's preoccupation with Marxism and its implementation, contemporary responses to his works are important. O'Neill, however, allows quotations from reviews to figure disproportionately in his analysis. By the same token, sales figures tell us something about the culture but little about the intellectual quality of the publications themselves. With respect to content, O'Neill's discomfort in the handling of Eastman's amorous meanderings is also evident in the treatment of the ideological course transversed in the works themselves. Perhaps as a result, comments intrude that reveal more about O'Neill's political preferences and mode of analysis than they do about the contours of Eastman's mind and the intellectual influences and tradition that shaped it. For example, O'Neill writes of Eastman's later years, "Had he not become so conservative, he might have completed his argument in this way. Socialism remains a beautiful dream. In real life it has been proven unnecessary by the welfare states of Scandinavia." To object to such judgments, whether stated explicitly as above or implied—as with the prefacing of a chapter on Eastman's criticism of Stalinism and Marxism during the forties with a quotation from *The Gulag Archipelago*—is not to ask for "value-free" history devoid of any trace of current preoccupations or personal commitment. Rather, it is to ask for the kind of thoughtful, pene-

trating analysis that allows us to understand and assess the man, his ideas, and his concerns and to do so in such a way as to enhance our grasp of larger issues, such as the nature of radicalism in America and the role of the intellectual in American life. Thus, while *The Last Romantic* may serve as an introduction to Max Eastman's life and thought, it will hardly suffice as the last word.

JANE DE HART MATHEWS
University of North Carolina,
Greensboro

MICHAEL DE L. LANDON. *The Honor and Dignity of the Profession: A History of the Mississippi State Bar, 1906–1976*. Jackson: University Press of Mississippi, for the Mississippi Bar Foundation. 1979. Pp. xi, 195. \$17.00.

The Mississippi Bar Foundation, the research arm of the Mississippi State Bar Association, commissioned Michael de L. Landon to prepare this modest history of the state bar. It is essentially an overview of the history of the association, largely drawn from its own published reports. Nonetheless, with the dearth of good material on the history of the state bar associations, it is welcome, despite its obvious limitations. To a great extent it would be more accurate to identify this book as a review of the annual meetings of the association and a survey of the major issues facing the association in the years covered. The author makes no attempt to present this work as definitive and in fact suggests that deeper and more thorough research is needed. At the same time, Landon is frank in showing the shortcomings of the association in dealing with such delicate issues as the rights of black Mississippians and prohibition. What one looks for in vain is any interpretation or attempt to relate the experience of the Mississippi State Bar Association to that of other state associations in the same region.

Similarly, this reviewer wishes that the opening pages on the predecessors of the Mississippi State Bar Association were more fully explored, as well as the activity of Mississippi lawyers in the American Bar Association from 1878 to 1906.

NORBERT BROCKMAN
Marianist Training Network

MICHAEL O'BRIEN. *The Idea of the American South, 1920–1941*. Baltimore: Johns Hopkins University Press. 1979. Pp. xvii, 273. \$16.00.

Michael O'Brien has attempted in *The Idea of the American South, 1920–1941* to explore why Southern intellectuals between the two World Wars "felt the need to express themselves in terms of a Southern

tradition" (p. 27). Such a topic is appropriate for intellectual history, and O'Brien raises our expectations for its fruitful pursuit with his introductory chapters, in which he seems not only to be aware of the multiplicity of Souths and southern identities but also to want to bring to them an interesting combination of historical, literary, and sociological methodologies. I particularly applaud his intention to relate the idea of the South to broad nonnational, nonsectional intellectual currents: Romanticism, Victorianism, and modernism.

Unfortunately, O'Brien only gives biographical sketches of six Southerners: Howard Odum, John Wade, John Crowe Ransom, Allen Tate, Frank Owsley, and Donald Davidson. Odum, to whom he gives the most detailed study, represents the New South; the others had connections with the Nashville Agrarians; all spent their careers in or in the shadow of the academy. Wade was a poor choice as the transition figure; William Alexander Percy would have served better. Each sketch ends abruptly at 1941, except that of Davidson, whose White Citizens Council activities apparently proved too tempting to be ignored. O'Brien explores Tate's idea of modernism and the dissociation of sensibility without examining Tate's most complete and least reduced embodiment of that idea and its relation to the South in his novel *The Fathers*. O'Brien fails to see the extent to which the South was primarily a metaphor for Ransom's essentially esthetic concerns. Indeed, of the Nashville group, only Davidson had a deep commitment to the Agrarian South as an actuality, a commitment expressed in social action that proved to be the source of his tragedy.

Such essays—and the ones on Odum, Wade, and Owsley are excellent—do not define the complex idea of southern identity. Their subjects represent only Georgia, North Carolina, Tennessee, and Kentucky. Missing are the "Deep South," the South of the Gastonia strikes, of Erskine Caldwell, of Broadus Mitchell, of Lilian Smith, and of Agee and Walker's studies. And a study of the idea of the South using, among others, literary sources but ignoring William Faulkner is lacking one of the forces that imprinted a special view of the region on Southerners and non-Southerners alike.

Furthermore, the ideational structure of the essays is marked by O'Brien's tendency toward Hegelian oppositions, without clear syntheses: Vanderbilt versus Chapel Hill, Agrarianism versus New South, regionalism versus nationalism, idealism versus atomization, race versus modernism. Such dichotomies have the ultimate effect of falsifying simplifications. The author's use of Karl Deutsch's abstract definition of nation and his use of Romanticism as though it had an exact and recognizable referent further weaken this work.

As an Englishman, O'Brien has, he thinks, "no partisanship on the competing moral claims of these various versions of Southern identity" (p. xi), a position that allows him to play down the central role of the Negro to a remarkable extent. He also has too limited a knowledge of the realities of the region whose idea he would define.

C. HUGH HOLMAN
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Chapel Hill*

PAUL BONNIFIELD. *The Dust Bowl: Men, Dirt, and Depression*. Albuquerque: University of New Mexico Press. 1979. Pp. xi, 232. \$12.50.

One of the bleakest and most enduring images of the Great Depression is the drought-stricken Great Plains. In this work Paul Bonnifield examines the heartland of the dust bowl, the Oklahoma and Texas panhandles and adjacent areas of Kansas, Colorado, and New Mexico. The author knows the region well and admires the people who withstood drought and depression and remained on the land. In researching their story he discovered a need to revise conventional views of the dust bowl.

Bonnifield shows that, when the depression struck, the area was still a frontier and was changing from diversified crop and livestock farming to a concentration on wheat. This shift was not caused by the high wheat prices of World War I, but came in the 1920s, primarily because of the mechanization that made large-scale production possible. Unfortunately, there was no concurrent advance in the methods of controlling wind erosion. Indeed, farmers, government, and scientific agriculturalists all completely overlooked the wind's potential danger until the dust bowl was well established. When wheat planting was curtailed after prices plunged in 1931, large uncultivated areas were left exposed to the ravages of wind and drought.

Although Bonnifield does not minimize the hardships of dust-bowl times, he points out that in many respects the area's business and agricultural conditions were no worse than those of other sections. His account of the dust bowl as an expanding petroleum frontier is particularly informative.

The author condemns most federal efforts to cope with the dust bowl, especially the land-use planning of the Agricultural Adjustment Administration and the Resettlement Administration, which aimed to remove submarginal land from cultivation and return it to grazing. He contends that the area's soils should not have been classified as submarginal and argues that wheat production is a viable economy for the region if practiced with strict wind erosion control. He credits dust-bowl farmers with developing techniques and implements for such control

in contrast to the poor record of the Soil Conservation Service. These arguments, backed by ample technical information, deserve consideration in the literature of the dust bowl.

Unfortunately, the book lacks perspective regarding the intent of federal policies. For example, the author sees the AAA, the RA, and the credit and relief agencies working deliberately to drive people off the land in accordance with a federal grand design for land use. This exaggerates the coordination of those agencies and also distorts the overall program of the Resettlement and Farm Security Administrations. There is also an erroneous assertion (p. 118) that the "Jerome Frank-Rexford Tugwell faction" gained control of the AAA. While Bonnifield is concerned with the local applications of federal policy, these local effects need to be seen in a broader context. Bonnifield's heavy reliance on local sources, truly a strength in much of his book, does not supply the needed overview.

On the whole this is an informative book on local dust-bowl conditions. It is generally well written and includes some good illustrations.

PAUL E. MERTZ
*University of Wisconsin,
Stevens Point*

GEORGE NORRIS GREEN. *The Establishment in Texas Politics: The Primitive Years, 1938-1957*. (Contributions in Political Science, number 21). Westport, Conn.: Greenwood Press. 1979. Pp. xiii, 306. \$22.50.

In this first thorough study of mid-twentieth-century Texas politics, George Norris Green begins by analyzing the historical sources of Texas conservatism: Texas nationalism and states' rights, the South Texas patrón system, the southern heritage of white supremacy and fundamentalism, the western heritage of individualism, and local elites. In the 1930s the state's business establishment strengthened these conservative tendencies as it achieved political dominance on the basis of new oil, banking, and insurance wealth as well as by public relations techniques. Yet in the early years establishment influence remained incomplete and sometimes ineffective.

Green devotes chapters to the state administrations and to the major campaigns of the period. Governors W. Lee O'Daniel, Coke Stevenson, Beauford Jester, and Allan Shivers all received business support but varied in accomplishment. Although O'Daniel proved inept, Stevenson and Jester showed moderation on some civil rights and labor issues. Shivers promoted public services, although primarily with consumer taxes, and supported Eisenhower for president to get the oil tidelands returned from federal to state control. In the 1940s

Texas businessmen opposed Roosevelt's renomination, financed organizations that fought civil rights and labor organization, sought to muzzle the University of Texas faculty, and formed the Texas Dixiecrats—but with only limited results. Congressman Martin Dies pioneered red scare politics later used by local elites in Houston and San Antonio. Business interests proved most successful in avoiding taxation. In the 1948 Senate race, which involved fraud on both sides, Green believes a fair count would have favored Stevenson over Lyndon Johnson. Shivers played upon fears of communism, labor, and civil rights to win reelection over liberal Ralph Yarborough in 1954. Green sees Texas politics turning away from emotional campaigns toward moderation in 1956 and 1957, when Johnson and Sam Rayburn kept the state Democratic Party loyal to the national party, Price Daniel was elected governor, and Ralph Yarborough won a Senate seat. In a final chapter, the author surveys Texas politics in the 1960s and 1970s. He concludes that establishment states' rights rhetoric of the forties and fifties represented a failure to meet responsibly the needs of the state.

Green has researched widely and his analysis is generally well balanced. While the author does not employ statistical methods, he draws upon such studies to conclude that Texas voting followed socioeconomic lines through 1956. Some topics, however, seem worthy of additional discussion, for example, the incipient Republican Party and the impact of religion and culture on Texas voters. Relationships within the establishment remain vague at times, so it is not clear why wealthy Texans divided and gave some support to liberal candidates such as Jimmy Allred and Ralph Yarborough. Finally, more comparison of Texas politics with those in other southern states would have enhanced the reader's perspective. Yet, if this volume is not the final word on all aspects of mid-twentieth-century Texas politics, it will be the basic study from which any future investigations must begin. Certainly it should stand with the better political studies of southern states in this period.

ALWYN BARR
Texas Tech University

GRZEGORZ BABIŃSKI. *Lokalna Społeczność Polonijna w Stanach Zjednoczonych Ameryki w Procesie Przemian* [A Local Polonia Community in the United States of America in the Process of Change]. (Biblioteka Polonijna, number 1.) Wrocław: Ossolineum. 1977. Pp. 207. 42 Zł.

This largely contemporary review of the small, heavily Polish suburb of Wallington, New Jersey, is a welcome addition to the growing bibliography of

Polonia communities. Similar to the recent studies of the group in Los Angeles by Neil Sandburg and the one in Detroit by Paul Wrobel, this work by a young Polish sociologist of the Polonia Research Institute in Cracow is a solid, meticulous, and comprehensive sampling of changing Polish-American generational attitudes and values in a town satellite to the industrial centers of Passaic and Clifton. Although the small size of his subject area—seven thousand Polish-Americans in a total population of twenty thousand—does handicap the author somewhat (he never adequately explains the selection of Wallington), the suburb does have a significance greater than its numbers would suggest. The community is really two ethnic settlements, one consisting of older working-class inhabitants who arrived around and just after World War I and another made up of a more affluent middle-class contingent who entered after World War II. Thus Wallington's Poles span the entire suburbanization process and Grzegorz Babiński utilizes the differential to advantage.

Babiński's conclusions are rather conventional, although perhaps not to his East European audience; his revision of Warner's social mobility thesis and Reisman's *Lonely Crowd* will not surprise American sociologists, historians, or current students of ethnicity. Polish ethnic cohesion has changed its character and composition rather than disappeared; institutions are no longer central to group identity, which is now more private, personal, and individual; intermarriage is more acceptable, although within rather than outside of Roman Catholicism; upward job mobility has occurred recently, particularly between the second and third generation, and younger members value education more than the earlier working class. Among other late ethnic critics, Andrew Greeley summarized many of these points in his recent *American Catholic* (1977).

American historians will find the work less valuable than sociologists, as Babiński ignores almost all of the important studies outside his discipline, although he uses many historical records and logically periodizes his review. Oddly, while an Old World academic, he has nothing to say about the European origins of his subjects' attitudes. His bias is evident, however, in referring to the pathology of assimilation, which is asserted rather than demonstrated.

Despite these limitations, this reviewer found two of Babiński's findings in particular well worth the effort in reading the work. First is his prediction (based in part on preliminary conclusions) that Polonia is at the threshold of a cultural, rather than political or nationalist, renaissance. Second, and particularly outstanding, is his tracing of how the group has been so adaptable to change, especially

to crises. He recounts the dramatic irony that Poles out of work survived the depression by starting family businesses—thus proletarians became bourgeois when capitalism nearly failed! Despite the regrettable absence of a map and due to a totally inadequate English summary, the work merits a full English translation for an audience that needs such ethnic community studies.

VICTOR GREENE
University of Wisconsin,
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BERT COCHRAN. *Labor and Communism: The Conflict that Shaped American Unions*. Princeton: Princeton University Press, for the Research Institute on International Change, Columbia University. 1977. Pp. xiv, 394. \$25.00.

The author, once an auto worker and UAW official and now associated with the Research Institute on International Change at Columbia, has written the most detailed and generally the most sophisticated book on this subject yet published. He does not argue the thesis implied by the subtitle. That is fortunate because, while the Communist Party certainly affected unions and unions affected the party, their relationship was not "the conflict that shaped" this country's unions.

The book's overwhelming emphasis is on the period from about 1929 to the early 1950s, years when the party's line changed frequently, usually only by a few degrees but in 1939 and again in 1941 by sudden and complete reversals of direction. The unions' problems, needs, and strategies also changed but were far more consistent than the party's. In the CIO's early days some unions welcomed Communists as experienced and dedicated organizers or at least warily tolerated them. Some unions were becoming anti-Communist even before the Nazi-Soviet pact, and, while the pact was in effect, tension between the principals became intense. After the German invasion of Russia and the Japanese attack on Pearl Harbor, the Communist Party played the role of Dr. Win the War so vigorously, even advocating a piecework wage system (the euphemism was "incentive pay") that many union leaders were furious about what they regarded as Communist subversion of labor's interest. When the Cold War began to take shape, union anti-Communism grew rapidly and by the early 1950s the party had lost almost all influence in the important unions. Bert Cochran splendidly describes the complexities and nuances of these party shifts and their effects on the relations of the party with the unions.

Other strengths are the brief interpretive observations scattered throughout the narrative, sometimes about party-union relations and sometimes about

other aspects of the labor movement. Many of these acute comments result from the author's association with the UAW and with working men. He is indeed a "labor intellectual." But the final chapter, an essay entitled "Postscript: Concepts of Labor Development," does not impress this reviewer. Its ambitions to be cosmic get it into trouble, and its petty criticisms of the Perlman thesis (p. 336) are at odds with what the author later writes (pp. 342-43), which is entirely consistent with what Perlman said in his classroom in the late 1940s.

As intelligent and useful as this book is, it has its flaws. The UAW was the CIO's largest union, but it receives disproportionate attention. Especially in the several sections on the auto workers, the reader has difficulty keeping his eyes on the forest because of the attention to the trees and even the leaves and branches. The style in general is adequate, and some sentences are wonderfully pithy and clean. Others, however, are pitiful humpbacked things that unnecessarily make the reader stumble and reach for his editorial blue pencil. Cochran also has a weakness for "literary" pretensions; some readers will be put off by the use of relatively uncommon French terms and such arcane allusions as, for example, a "Gadarene plunge down the abyss" (p. 132). Further, the absence of a bibliography is a real nuisance; the backnotes do not even provide full bibliographical information after the first citation.

Insufficient perspective is a most difficult problem for historians of the recent past. For the most part Cochran avoids the language of partisanship that often appears in the writings of historians who were contemporary observers of events treated in their works. He is often critical of major party leaders and important unionists, whether anti-Communist or not. For example, neither Browder nor Reuther escapes censure. Yet one closes the book with the feeling that the Communists receive the harsher treatment.

Cochran has written the most emotionally detached book on this subject yet to appear, but there still is some distance to go. Probably it will be a long time before an American will be able to be dispassionate on this part of our history. Meanwhile, it would be interesting to read the efforts of someone from another nation.

DAVID A. SHANNON
University of Virginia

GEORGE Q. FLYNN. *The Mess in Washington: Manpower Mobilization in World War II*. (Contributions in American History, number 76.) Westport, Conn.: Greenwood Press. 1979. Pp. xi, 294. \$17.95.

In 1944, when President Franklin D. Roosevelt hinted that Paul V. McNutt might make a good

running-mate in that year's election, Secretary of the Treasury Henry Morgenthau replied, "I will crawl from here to the Capitol on my stomach and back again if it will keep you from taking McNutt." Apparently, neither Roosevelt nor Morgenthau took the other's suggestion seriously. George Q. Flynn's study of wartime manpower mobilization helps explain why there was never any chance that McNutt would be the nominee: as Director of the War Manpower Commission he had by 1944 alienated too many powerful interest groups and political leaders.

Flynn describes the considerations that led to the creation of the War Manpower Commission in April 1942, the work it did, and the problems it faced. He offers an appraisal of the controversies over the "work or fight" order, national service, the deferment of farm workers, the proposed "father draft," and reconversion. In addition, Flynn discusses the limited role McNutt played in combating job discrimination against blacks and women. The author dissects the relationships between the WMC and various pressure groups, particularly labor unions and the armed services, that had a direct stake in manpower policies, and he lays bare the truly Byzantine relationships between McNutt and several bureaucrats, particularly Secretary of War Henry Stimson and Secretary of the Interior Harold Ickes, with whom he had to contend.

Flynn maintains that McNutt's policies were based on a belief in voluntarism and localism. He "combined an almost reactionary commitment to the preservation of traditional values with a vision of the war as a new beginning" (p. 12). This commitment limited the impact of manpower mobilization, and nowhere was this limitation clearer than in the case of blacks and women. Manpower policies "pushed blacks into the unskilled laboring class"; indeed, McNutt "played a major role in this campaign of smothering the black drive for equality" (pp. 167-68). Similarly, the WMC did little to contradict the common assumption "that women should not be considered equal to men in economic matters" (p. 173). Yet Flynn concedes that the failure to achieve "reform" was "almost inevitable" given the power of organized farmers and workers, the widespread prejudice against blacks and women, the conservative pulse of Congress, and the special pleadings of the Selective Service System. In view of these constraints, Flynn concludes, "what seems remarkable . . . is that McNutt achieved anything worth remembering" (pp. 255-56).

While the author has a thesis, he does not push it further than his evidence allows. That evidence is drawn heavily from a wide range of manuscript collections, including the diaries of Ickes and Stimson, the papers of McNutt and the War Manpower Commission, and the files of Selective Service Di-

rector Lewis B. Hershey. Flynn has provided an informed and thoughtful account, one that nicely supplements Albert Blum's *Drafted or Deferred* (1967) in explaining the political and social, as well as the economic, dimensions of manpower mobilization during World War II.

RICHARD POLENBERG
Cornell University

MONTE M. POEN. *Harry S. Truman Versus the Medical Lobby: The Genesis of Medicare*. Columbia: University of Missouri Press. 1979. Pp. xii, 260. \$20.00.

This volume is an examination of Harry Truman's efforts to obtain a national health insurance program and the factors behind his inability to do so. Principally, that failure was the result of two matters. One was opposition from the American Medical Association and the allies that organization generated. A second factor was the general climate of public opinion.

In the former case, the AMA once favored compulsory health insurance, but that ardor cooled rapidly in the five years after World War I. By the Truman era this country's richest and most influential post-World War II lobby had taken the position that all such schemes were "socialistic" and "foreign" to the American system of government—indeed were incompatible with protection of the basic liberties of the American people. The AMA-led opposition coalition was sufficiently powerful that the likelihood of gaining enactment of a national health insurance program was virtually nil.

The general climate of opinion reinforced and provided sustenance for that opposition. By 1949 the extreme, indeed frantic, anti-Communist mood that dominated the American scene meant that any legislative proposal that could be identified with the word "socialism" was immediately suspect. National health insurance was so defined by its enemies. Truman was a realist. This state of affairs, combined with the Korean War and unfavorable returns in the 1950 fall elections, persuaded him to move toward a less ambitious plan for government health insurance, one that would apply essentially to the aged. While this decision obtained no real result during the Truman years, Monte M. Poen understands it to mark the beginning of the long legislative battle that reached its culmination with the enactment of Medicare in 1965. This story, notes the author, is not, therefore, one of defeat but, at least in the long run, one of victory—a highly debatable interpretation.

Poen to the contrary, this aspect of Truman's administration can easily be read as a chronicle of failure. If Truman was the first president to provide health reformers with a mandate to act, that man-

date had little consequence. To understand Medicare as the belated conclusion of Truman's health-care efforts is to acknowledge a tragic history of the medical lobby's continuing ability to deny Americans the medical protection they deserve and to point up anew the highly restricted perception of what constitutes appropriate protection that has long prevailed in Washington. In the latter, Harry Truman was no exception. His health-care vision was woefully inadequate for the needs of the time.

This volume is well researched and well written. Despite my quarrel with Poen's perspective on Truman, I regard the work as a valuable contribution to the history of the Truman administration as well as to the history of public health. With respect to interpretation, however, I suspect that many scholars will find, as I do, that the author's depiction of Truman is far too favorable and the case for that view much too weakly drawn.

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CEDRIC BELFRAGE and JAMES ARONSON. *Something to Guard: The Stormy Life of the National Guardian, 1948-1967*. New York: Columbia University Press. 1978. Pp. ix, 362. \$19.95.

Something to Guard: The Stormy Life of the National Guardian, 1948-1967 is as much a history of the struggles of the American left in the postwar period as it is of that particular publication. The authors, Cedric Belfrage and James Aronson, have written an account that highlights their successes and failures as editors of the *National Guardian*. This very personal account reflects a point of view critical of accepted governmental actions. For the almost twenty years of association between the authors and the paper, the *National Guardian* published despite financial problems, governmental harassment, and the deportation of Belfrage. It served a distinct public but provided an alternative to the news that was supplied to the American people.

The paper was often criticized for being uncritical of the Soviet Union, and for that reason it alienated great numbers of liberals as well as conservatives. Belfrage directly confronts these charges by explaining that the policy was to focus on the activities of the American government. He does add, however, that in retrospect charges of repression within Russia should have been given more attention by the staff. The basic dilemma revolved around the fact that the Soviet Union was also the great hope for socialism in the world. Thus, the ideological attachment to a Communist state prevented a more critical stance. In fact, this proved to be a great problem for James Aronson, who left the

National Guardian's editorship in 1967 because of continuing difficulties with and criticism from members of the "New Left." The New Left of the 1960s was much less ideologically oriented toward the Soviet Union than the so-called Old Left of the 1940s.

Despite troubles, the paper had an extraordinary record. It fought against Joseph McCarthy in a way that no mainstream publication did. It highlighted the impending American military involvement in Vietnam long before others paid attention to it. Incidents involving race relations, discrimination, and injustice were constantly addressed. And the *National Guardian* fought unrelentingly to save the Rosenbergs from execution.

The rewards for this work were mixed. By championing causes, the publication was totally involved in presenting its point of view, which was its stated goal. Other goals, however, were never achieved. Aronson, Belfrage, and the *National Guardian's* third executive, John T. McManus, had wanted the paper to become a financially successful, independent publication with a sizable circulation. This never came about. After some initial financial help from Anita McCormick Blaine, the paper was always plagued by monetary problems. The total number of readers also never lived up to the expectations. In fact, in 1967 the paid circulation of the paper was twenty-eight thousand, a far cry from the hundreds of thousands that had been a one-time dream.

In retrospect, although the *National Guardian* never achieved its great readership, it served an important purpose. It challenged the conventional wisdom of the times and left a record of honorable dissent in a time when there was too much conformity.

ALLEN YARNELL
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Los Angeles*

ROBERT H. CONNERY and GERALD BENJAMIN. *Rockefeller of New York: Executive Power in the Statehouse*. Ithaca: Cornell University Press. 1979. Pp. 480. \$15.00.

For critical or blatantly hostile studies of Nelson Rockefeller, scholars should turn to the prolific accounts by journalists and investigative reporters. Robert H. Connery and Gerald Benjamin's examination of his governorship has offered something more enduring: an analysis of the state of American federalism. As in an anthology they edited for the Academy of Political Science, this book uses New York as a prime example to ponder the ability of state governments "to find solutions to the societal problems" that once inspired remediation from an optimistic nation (p. 18).

The Connery and Benjamin work does for feder-

alism what earlier state and local studies did for American constitutional development, the Civil War and Reconstruction, and, more recently, the New Deal. They accept Donald M. Rober's concept of "positive liberalism" and gladly apply that label to Rockefeller. In a series of well-written chapters, the governor emerges as a master builder and "active-positive" executive. His deep concern for the revitalization of state government amply warranted apprehensions that he was a "big spender." His 1958 election marked a "turning point" in placing "aggressive, problem-solving style in New York's constitutionally strong governorship at a time when state government generally was entering a period of enormous growth and change" (p. 38). Where the federal government lagged, Rockefeller's New York set the pace for medical insurance programs, aid for mass transit, water pollution control, park development, housing, and cultural programs. Converting a moribund state university into a major provider of quality public education was, by most accounts, his major achievement.

Still, the authors argue that politics and federal-state relationships imposed severe restraints on even New York's activist governor. Contrary to other writers, Connery and Benjamin contend that Rockefeller's powers were far from feudal; he did not quite own the legislature. The forty thousand patronage jobs were not all *his* to dispense. The authors, both political scientists, minimize the relationship between Rockefeller's personal fortune and his office, but they do point out the advantages made possible by his great wealth. When trying to get money from Washington, however, he encountered frustration over revenue sharing and was forced to do battle through bipartisan, mutual-interest alliances. Ignoring David Horowitz and Peter Collier's point that Rockefeller's ultimate ambition was thwarted by the tendency of heartland Republicans to see him as the personification of Eastern financial power, Connery and Benjamin argue that he was undone because the conservative, not the liberal, critique was finally decisive. The governor tried to do too much too quickly. Perhaps the final irony is that his passion for building was more easily satisfied in Albany than it could have been in Washington.

The authors relate Rockefeller's reluctance to speak out on Vietnam and Watergate to his need to ensure a friend for New York in Washington. Still, despite wanting the federal government to transfer services to the state, he opposed cutting defense budgets. In tracing his rightward shift, they place more importance on the governor's perception of what was politically feasible for the benefit of the state rather than on his own ambitions. Yet, for New York, Connery and Benjamin acknowledge that the governor's legacy was a budget thrown

askew by the necessity for "back-door" financing. Even the nation's wealthiest state could not solve human needs by "going it alone." Although too often reading like an in-house report of accomplishments, Connery and Benjamin's examination raises important questions for historians trying to fathom the dynamics of American federalism.

HERBERT S. PARMET

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JOHN P. LOVELL. *Neither Athens nor Sparta? The American Service Academies in Transition*. Bloomington: Indiana University Press. 1979. Pp. xviii, 362. \$17.50.

For most civilian scholars the American service academies are probably some combination of riddle, enigma, and puzzle and about as appealing as a Tolkienesque land of wizards and monsters. For John P. Lovell, a professor of political science at Indiana University, the history, contemporary policies, and prospects of the Military, Naval, Air Force, and Coast Guard academies have been career-long interests. *Neither Athens nor Sparta?* presents his years of research in comprehensive, coherent fashion.

Particularly because his study is done on a comparative basis and subtly mixes history and organizational theory, Lovell's work is in a class above the normal drivel written about the academies. His book should interest military historians, scholars of American higher education, and students of organizational change. Solidly based on archival research and interviews and enlightened by the author's sympathetic (but critical) understanding of the American military establishment, *Neither Athens nor Sparta?* is an intellectual achievement of the first order.

Lovell's thesis is that all the military academies share a common problem in self-definition: throughout the twentieth century they have defined their role as providing junior officers with (1) an excellent undergraduate technical *and* liberal education; (2) personal or "character" training uniquely suited for military leadership roles, especially in war; and (3) the rudimentary skills of lieutenants and ensigns who can go quickly to tactical units. The only problem—discerned by most students of the academies—is that modern military technology has not yet produced either a forty-eight hour day or the organizational wisdom to balance the academies' contradictory goals without conflict.

After reviewing the development of the academies until World War II, Lovell concentrates on the special tensions and incremental reforms that have characterized the decline of the seminary-academy model for the federal military college. Students of

higher education will notice parallels with the changes in church-related liberal arts colleges and large public universities occasioned by cost, sheer growth in student body and faculty, and external cultural pressure, largely produced by urbanization and the rise of the self-indulgent, secularized middle class. Lovell focuses on two specific academy problems, the liberalization of academy curricula and pedagogy and the constant struggle to balance the time demands of the classroom and the drill field. When one mixes in the "duty, honor, country" rhetoric—put into actual operation in the Honor Code with a doggedness that would impress The Three Hundred Spartans—the predictable result has been the cheating scandals and the pervasive frustrations and disappointments that mark the academies' recent history. Although a series of academy superintendents have shown great moral courage and ingenuity in producing reforms that appeased both their internal and external clients, Lovell is particularly good in explaining why the reforms are so limited. "Heroic" leaders may guide an academy through a particular crisis and direct an enlightened immediate "clean-up," but the dichotomy between the Athenian and Spartan models is too great to accommodate deep reform.

In his conclusion, Lovell examines the alternative models for reforming the academies that have been suggested by other students of American officer education. Although Lovell believes that policies followed by the Air Force Academy (the introduction of academic majors) and the Naval Academy (the civilianization of faculty) will provide models of change for the other institutions, he believes it more likely that the academies will cling tenaciously to their military character. The academy military administrators and faculty may hold ever-larger numbers of civilian degrees (and perhaps civilian ideas), but at least some of them know which side eventually won the Peloponnesian Wars.

ALLAN R. MILLETT
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SARA EVANS. *Personal Politics: The Roots of Women's Liberation in the Civil Rights Movement and the New Left*. New York: Alfred A. Knopf. 1979. Pp. xii, 274. \$10.00.

In a book with great conceptual and methodological strengths, Sara Evans has given us an effective and fascinating analysis of the origins of a social movement. She traces the development of the women's liberation movement, the "second wave" of American feminism, from the experiences of women in earlier social movements. The book covers, in historical order, southern black, southern white, and then northern white women in the civil

rights movement; the impact of black power on all of them; and women in the student, community organizing, and antiwar movements of the 1960s. The questions she asks come in part from the analogy to the development of the first wave of feminism out of the abolition and other radical and perfectionist movements of the pre-Civil War period. These informed questions led Evans to a more complex thesis about the relation between feminism and the New Left than has heretofore been common: women were oppressed and subordinated, treated disrespectfully and used exploitatively in the left, and got angry as a result; but from the left women also gained organizational skills, self-confidence, political passion, and a language in which to think about and express their own grievances.

Methodologically, Evans's book is an example of good use of oral history. Most recent "oral history" has been exclusively biographical, and too much of it only barely transcends the level of anecdote, neglecting the analysis, synthesis, and critical evaluation of evidence that transform stories into history. Evans's interviews remain evidence checked against documents and other interviews and ultimately put together to make a coherent and convincing explanation of the collective experience of her subjects. Some of her most powerful findings emerge from discrepancies between the subjects' memories and the documents: a telling illustration of the status of women in the SDS, for example, is that one male leader could not recall the name of a single woman attending a key conference, although the minutes and papers presented revealed several women he knew well had played important roles. The integration of "personal" experience that would not be available in written documents with political events provides unusual insight into the motivations that propel a social movement like feminism. Thorough research and careful interpretation allow Evans to synthesize events not far enough in the past to allow the leisurely hindsight upon which most historical writing depends.

The weaknesses of the book are primarily in two areas. First, it does not connect the particular experiences of the relatively small, though influential, group of women who are its subjects with the socioeconomic changes that affected the general population of women in the U.S. in the relevant period. The first and last chapters, which attempt an overview of factors such as women's employment, family, and demographic change are thin, lacking in complexity, and neglect much new scholarship. The failure to use social class as a category of analysis, both in the background chapters and in the evaluation of the factors that propelled women toward feminism, is a damaging omission.

A second area of weakness results from the author's failure to treat the *ideas* of women's liberation,

specifically the intellectual and political relation to feminism to the New Left. Thus, for example, she presents the tendency of the women's movement toward decentralization, variety, and short-lived groupings as an organizational failure (pp. 222-23). Closer attention to feminist thought, or New Left political thought, would have revealed that this tendency also represented a critique, however inarticulate, of the authoritarianism and hierarchicalism of preceding political organizational forms.

Such weaknesses should not deter readers from the opportunity to learn, both methodologically and substantively, from this remarkable history of a contemporary social movement.

LINDA GORDON
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Boston

YSABEL RENNIE. *The Search for Criminal Man: A Conceptual History of the Dangerous Offender*. (The Dangerous Offender Project.) Lexington, Mass.: Lexington Books. 1978. Pp. xix, 345.

The Search for Criminal Man, a volume in the Dangerous Offender Project, grew out of Ysabel Rennie's commission to produce an annotated bibliography of the principal works dealing with "dangerousness and criminality." Her study became a full-scale summary of ideas about the dangerous offender and the criminal man in Western society from ancient times to the present, with major emphasis on the nineteenth and twentieth centuries. Rennie does an effective job of tracing and classifying theories of criminal behavior over the centuries and from one writer or school of thought to another. Not surprisingly, the book lacks depth of treatment; considering its breadth, the amount of information packed into 275 pages is impressive.

The author notes that what is "criminal" tends to depend on what society considers to be dangerous. According to contemporaries, witches were dangerous criminals during one period, heretics during another, and members of workingmen's societies during yet another. As definitions changed, those activities considered to be dangerous became "criminal." Crime therefore is a natural aspect of society, and Rennie theorizes that justice (rather than an end to crime) ought to be a major social objective. Her historical examination includes those methods by which upper classes traditionally exempted themselves from punishment as well as documentation of the ineffectiveness of severe punishment as a deterrent to crime. Differentiating between American criminologists, who emphasize environmental factors, and earlier European positivists, who found the causes of criminality within the offenders, Rennie concludes that the factors producing criminals are unknown and probably unknowable.

A major shortcoming of the book is the absence of any discussion of organized crime or the professional criminal. The criminal entrepreneur whose criminal activities are his vocation is missing from Rennie's scheme of things. Although criminologists in recent years have taken organized crime into account, Rennie has not attempted to explain the activities of people like Meyer Lansky and Al Capone. This omission is ironic because Rennie quotes Capone in her introduction but does not again mention him or the professional criminal.

As Rennie herself points out, hers is a study of the "dangerous offender," one who poses a danger to society. Society generally views as dangerous the lawbreaker who engages in street crimes, not the syndicate entrepreneur who makes book or deals in stolen securities as an illegal service to a portion of society. Unless society defines as "dangerous" those who perform wanted but illegal services, the problems created and sustained by professional criminals will persist.

Although not innovative, the book is a piece of solid scholarship, potentially of considerable value to criminologists, sociologists, political scientists, and historians.

HUMBERT S. NELLI
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DAVID HALBERSTAM. *The Powers That Be*. New York: Alfred A. Knopf. 1979. Pp. 771. \$15.00.

David Halberstam's latest book, he tells us, is about "the rise of modern media and their effect on the way we perceive events" (p. 739). He focuses on CBS, *Time*, the *Washington Post*, and the *Los Angeles Times*, plus numerous references to the *New York Times*. The book is based on what the dust jacket claims as "nearly a thousand interviews." I counted 501 names in the acknowledgments; Halberstam says this is "about 95 percent" (p. 740) of those with whom he spoke. He says he read extensively in secondary literature, particularly Erik Barnouw's *History of Broadcasting in the United States* (1966-71).

When *The Best and the Brightest* (1972) appeared, it was criticized for being overlong, pointless, and gossipy. Halberstam's new book has the same defects; the text could be cut by two-thirds with no loss. Factual details are sometimes repeated four times in ten pages. The absence of a developed thesis, lack of proper documentation, and numerous errors of fact for events before the 1960s suggest that historians will have to use this book with caution. There is much here that might be true and, if true, valuable, but there is also no certainty that sloppy research does not undermine the very parts that seem most interesting.

It is instructive to compare Halberstam's treat-

ment of Vietnam in *The Best and the Brightest* with what he says here, for it reveals a major problem of interpretation. In 1972 he scarcely mentioned television; *The Powers That Be* hardly mentions any political figures save for presidential campaigners. The Vietnam war cannot be comprehended solely from potted biographical sketches of political and military leaders; the media is more than the hirings and firings of reporters or the illness of "Phil" Graham. In 1972 Halberstam claimed that Clark Clifford persuaded Johnson not to seek re-election in 1968; now he talks about Walter Cronkite. Others will have to figure out who influenced whom, though Herbert Schandler's *The Unmaking of a President* (1977) is a fine beginning.

Halberstam eventually tells us that the media, now great bureaucracies, have lost a measure of integrity; he also quotes Lyndon Johnson, who believed that television had "broken all the machines and the ties between us in Congress and the city machines" (p. 6). Halberstam feels that television has become too powerful and that television presidents have too much power, but there is little evidence. Endless tales of reporters getting scoops or editors getting old are not the way to demonstrate how the media have changed the shape of American society.

This book also contains important information. What Halberstam says about television's coverage of the war in Vietnam (pp. 507-14) and about Johnson and television (pp. 6, 139, 428-38) is highly suggestive. And future students of television should find the intuitive assessment of William Paley useful. Halberstam dislikes Paley intensely and has gone to great lengths to dig up dirt. What he says about Edward R. Murrow is conventional and incorrect, but I feel there is truth in the portrayal of Paley. Readers may want to compare Paley's own version in his *As It Happened: A Memoir* (1979).

DAVID CULBERT
Louisiana State University

CANADA

SERGE GAGNON. *Le Québec et ses historiens de 1840 à 1920: La Nouvelle-France de Garneau à Groulx*. (Les Cahiers d'histoire de l'Université Laval, number 23.) Québec: Presses de l'Université Laval. 1978. Pp. 474. \$17.50.

The author's project was not an easy one to realize. Serge Gagnon planned "dégager les rapports d'influence entre la connaissance historique et la société canadienne-française au cours des années 1845 à 1920" (p. 1). He hoped to take the approach of a social scientist looking at written history as a privileged documentary source on the characteristics of a given society at one moment of its evolution and as

a powerful instrument of regimentation in the hands of the governing classes. His attempt was a daring adventure in the intellectual and social history of French Canada for almost three generations.

The plan followed by the author did not help him. The introduction is both an essay on epistemology as applied to historical knowledge and an endeavor to explain how the Roman Catholic Church succeeded in establishing its powerful influence over French Canadian society after the 1840s. The conclusion itself partially repeats the introduction. These two parts are nevertheless incomplete, because the problems dealt with are so numerous and so complex. Only readers already familiar with French Canada's history will understand the author's interpretation and viewpoints.

There are four chapters devoted to different groups of historical works: hagiographies; hero-worshipping biographies; specialized syntheses; and general syntheses. If Faillon's works rightly belong to that of a hagiographer, Auguste Gosselin's *Vie de Mgr de Laval* should be considered a biography. Many readers will be surprised to discover that Benjamin Sulte's *Histoire des Canadiens français*, this collection of forays into French Canada's past, is promoted as a general synthesis in company of Garneau's *Histoire du Canada*, a landmark both in French Canadian and North American historiography. Sulte does not deserve so much honor, but Gagnon has chosen him as a martyr for the cause of liberal and lay thinking in the Church-dominated French Canada of the late nineteenth century. The author limits his analysis of Groulx's approach to New France to his *Naissance d'une race*, an incomplete and unjust treatment of this prolific and many-sided historian.

The main weakness of the book comes from the author's attempt to give his readers all of the fruits, both the ripe and the unripe ones, of his long and original research begun as a doctoral dissertation. Scholars interested in the historiography of French Canada, however, will gather valuable bits of information in Gagnon's book, and many of his commentaries will provoke fruitful reflections and reactions. He has opened new vistas in French Canadian intellectual and social history. His contribution would have been greater if he had chosen a more functional plan and streamlined his global approach to history.

MICHEL BRUNET
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THOMAS FLANAGAN. *Louis 'David' Riel: "Prophet of the New World."* Toronto: University of Toronto Press. 1979. Pp. ix, 216. \$15.00.

Most of the extensive literature, scholarly, popular, and fictional, dealing with Louis Riel and his two

abortive rebellions, 1869–70 and 1885, concentrates on the political character of the man and his movement. Controversy turns on two topics: the onus of responsibility for the rebellions and the question of Riel's mental condition. Contemporaries were divided on both questions. Most French Canadians held the Macdonald government responsible for the western troubles and contended that their "brother Riel" deserved clemency on grounds of insanity. English Canadians, for the most part, condemned him as a murderous revolutionary who stood in the way of Canada's natural claims to the prairie west. They demanded, and got, his execution. Though historians have remained divided, the weight of opinion, especially popular and liberal-left opinion, has come to view Riel as a martyr even while judging him insane. Yet Riel himself fought the charge of insanity harder than he fought anything. What for others was insanity was for Riel a sense of religious mission, a mission to establish a new religious state in the West, ruled by a new pope, the ultramontane Bishop Bourget of Montreal.

Thomas Flanagan, a political scientist, has chosen to take Riel at his word. He virtually ignores the questions that have long vexed historians and instead examines the *métis* leader as a religious thinker and actor. This is a fruitful and mainly convincing approach. The book is a thorough and balanced examination of the tortuous, confused, chiliastic writings and rhetoric of the "Prophet of the New World."

Flanagan has done an exceptionally able job of piecing together what often seem the incoherent ravings of a mind near the end of its tether. He demonstrates considerable knowledge of Biblical history and millenarian thinking and makes Riel's ideas less outlandish than they have often appeared. Still, many readers will find in this book the final proof of Riel's disturbed mental state. Occasionally one grows impatient, as Flanagan labors to make sense and pattern out of the jumble of visions, nightmares, and sometimes self-serving imaginings of his subject. But, in the end, he does make the clerico-conservative and theocratic cast of Riel's heretical mind more comprehensible than any previous writer.

But the book is not without its weaknesses. First, Flanagan's understanding of Quebec ultramontan-ism is superficial and inadequately researched. Second, he fails to pursue the evidence he has turned up concerning Riel's sexual troubles. A little psychohistory might have supplemented the theology here.

But the most serious criticism must be laid against Flanagan's laudable effort to relate the *métis* rebellion to other millenarian movements. The problem is twofold. First, Flanagan, though he makes a convincing case for Riel as a religious

thinker, fails to show any extensive influence of Riel's religious ideas on his followers. Their interests were much more mundane. Second, Flanagan looks in the wrong place for parallels—to Norman Cohn's medieval millenarians and contemporary Rastafarian-like phenomena. He mentions the Sioux Ghost Dance only in passing, though in time and place it was very close to Riel, and he completely ignores the Handsome Lake Religion, so brilliantly examined by A. F. C. Wallace. Indeed, a consideration of Riel within the framework of revitalistic movements as set out by Wallace years ago might have transformed Flanagan's useful corrective to the standard accounts of Riel into a work of truly imaginative scholarship.

RAMSAY COOK
York University

R. D. CUFF and J. L. GRANATSTEIN. *American Dollars—Canadian Prosperity: Canadian-American Economic Relations, 1945–1950*. Toronto: Samuel-Stevens. 1978. Pp. 286. \$14.95.

Historians should not infer from the title of this slender but significant monograph that its utility is confined to specialists in Canadian-American relations or to students of Canadian history, particularly those concerned about the responsibility of Liberal governments for United States domination of Canada's economy. Scholars interested generally in the diplomacy and economics of the period will find it both informative and provocative. The chapter entitled "Canada and Containment" not only shows how Canadian officials skillfully exploited anti-Soviet anxieties then prevalent in Washington to direct the United States into multilateral association whereby they might influence American policies but also dissects the debate in Ottawa over the motivations and objectives of Moscow's behavior, a debate that featured the variant analyses of Escott Reid, Maurice Pope, and Dana Wilgress. Similarly, R. D. Cuff and J. L. Granatstein demonstrate how these same officials used the Korean War to further the desired ends of American commitment to a Europe-First strategy and a program of rearmament with weapons procurement in Canada.

Primarily, however, the book details the interaction between officials in Washington and Ottawa as they sought to solve the problems resulting from the economic decline of Great Britain and Western Europe and the emergence of the United States as the world's most powerful economic nation. Critical to Canada were the immediate postwar shortage and continued loss of American dollars and, because of the increasing north-south commercial relationship, an unfavorable trade balance that chronically threatened its dollar reserves. Drawing

largely on previously unexplored government documents, private collections, and interviews with surviving principals, the distinguished authors effectively portray the lobbying skill of Canadian diplomats and a sensitive but politically cognizant American executive as they develop such topics as the imposition of selective restrictions on imports from the United States, off-shore purchasing under the Marshall Plan, the abortive Canadian-American customs union discussions, the southward movement and stockpiling of strategic minerals, Washington's agreement to procure arms in the dominion, and Ottawa's resistance to American pressure for a greater financial contribution to European recovery once the Canadian economy turned upward. Their presentation of Canadian negotiations with the growing imperial power of the United States is traditional: they paraphrase or deftly weave into the text quotations from memoranda of conversations, dispatches, and position papers, an approach that affords readers a glimpse of bureaucratic policy making.

The authors or editors occasionally slipped. Senator Milton Young is introduced as a Nebraska Republican. Several grammatical errors escaped detection. A line was omitted on page five. Robert Taft is termed an isolationist when "unilateralist" might have been a more accurate appellation. Perhaps they quote too frequently and at too great length from the documents. Perhaps, also, they could have developed more meaningfully the nationalistic overtones in Congress and the divisions between the Departments of State and Agriculture as they affected Canada. These minor criticisms aside, the authors have produced another cogent study, one that will stand until additional documents become accessible and provide a different perspective from which to view these problems and the penetrating questions Cuff and Granatstein raise.

RICHARD N. KOTTMAN
Iowa State University

LATIN AMERICA

ANDRE GUNDER FRANK. *Mexican Agriculture, 1521-1630: Transformation of the Mode of Production*. (Studies in Modern Capitalism.) New York: Cambridge University Press. 1979. Pp. xiv, 91. \$13.95.

Andre Gunder Frank has written an exercise in right thinking. He would correct erroneous interpretations of the nature of the Mexican economy and society during the sixteenth and seventeenth centuries by François Chevalier, Eric Wolf, myself, and others. He agrees that there was a shift from a pre-Conquest to a colonial, Hispanized economy

but objects to the concept of a dual economy and substitutes that of a single economy tied to the world market. He agrees that the Spaniards appropriated much land for haciendas and that these haciendas ultimately derived labor through debt peonage. He objects strongly to characterization of the haciendas as feudal, in Marxist usage, holding that they were capitalist. He further objects to the concept of depression in the later sixteenth and most of the seventeenth centuries and counters that these were years of prosperity in which the large estates developed in response to brisk demand and rising prices.

The entire essay is seriously flawed in two ways. In the first place, much of the controversy Frank wants to excite or settle rests upon definitions. What, for example, is meant by dual economy or unified economy? The Indians obviously were organized differently from the Spaniards but contributed production and labor to the Spanish sector. Neither Chevalier nor I use the term feudal in the Marxist sense. Use of the term by others means that the haciendas are considered exploitive. Chevalier does see the hacendado in the North as a leader rallying his employees for defense against the Indians and dispensing a rough justice. Depression or prosperity become also very much a matter of definition. Frank does not dispute that Indian agricultural production fell drastically; I do not dispute that Spanish production rose. That the increase in the latter could compensate for a drop on the order of nine-tenths in the former would require demonstration. Chevalier's concept of a retreat into agriculture applies to the North, where mining did contract and could hardly provide an expanding market.

In the second place, a great deal of fine detailed study, based on archives and business accounts, is altering our knowledge of these questions. Frank, as he admits (p. xi), is aware of the new work but has not taken it into account. So he writes of occupation of land without reference to Lesley Simpson's fundamental study of 1952, argues the role of mining without Peter Bakewell's fine study of Zacatecas, bases his references to sugar raising on Sandoval rather than Ward Barrett, appraises the Valley of Oaxaca without knowledge of William Taylor, and ignores new studies of haciendas, Indian communities, and the question of dual economy and society.

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WILLIAM B. TAYLOR. *Drinking, Homicide, and Rebellion in Colonial Mexican Villages*. Stanford: Stanford University Press. 1979. Pp. 242. \$16.50.

William B. Taylor's *Landlord and Peasant in Colonial Oaxaca* (1972) and his prize-winning article, "Estate and Class in a Colonial City: Oaxaca in 1792" (1977), co-authored with anthropologist John K. Chance, have established him as an authority on colonial land patterns and society in southern Mexico. In his present work, Taylor again focuses on colonial Oaxaca but has broadened the scope of his research to include Central Mexico.

Drinking, Homicide, and Rebellion examines social behavior in Indian peasant communities during the late eighteenth century. It is based primarily on archival research in several *ramos* of the *Archivo General de la Nación* (Mexico), state and municipal records in the Valley of Oaxaca, Inquisition trials, civil litigation, and documents relating to Indian affairs. Printed sources in the bibliography also reveal the author's familiarity with a wide range of literature dealing with the history, sociology, and anthropology of peasant communities throughout the world.

The first of four chapters provides the colonial setting and geographical limitations of the study. A total of twenty-nine *alcaldías mayores* (administrative units) and seventy-two towns in the intendancy of Mexico and the region of Antequera (Oaxaca) comprise the loci of Taylor's study. These are probably the best examples of Mexican colonial experience, for they include the heartland surrounding Mexico City as well as the important geographical and cultural subregions that lie within the Valley of Oaxaca. Separate chapters are devoted to the incidence of drinking, homicide, and rebellion within the two areas.

In his conclusions on drinking in peasant communities, Taylor questions that a "plague of alcoholism" swept the Mexican countryside after the conquest. The anguish and despair of the displaced Aztec elite, well documented in the *Florentine Codex* and *The Broken Spears*, did not necessarily produce a commensurate malaise among the lower classes who sought relief in the consumption of intoxicants. Evidence compiled by Taylor does suggest that Indians drank more and drank more often after the conquest, but he does not find conclusive evidence of unrelieved drinking as an escape from social pressures.

Homicide appears to have been prompted more by personal animosity than by group consciousness, class war, or political motivation in both areas, although intracommunity homicide was less prevalent in Oaxaca. Rebellions in rural communities of both regions were essentially at the same level, but were far less common than in the post-Independence era. The crises in national leadership that spawned the revolts of 1810 and 1910 appear to have been absent in the eighteenth century.

Taylor's painstaking research and careful conclusions make this book required reading for Latin

American scholars. It also forces us to reconsider our generalizations about colonial Indian society.

DONALD CHIPMAN

North Texas State University

OAKAH L. JONES, JR. *Los Paisanos: Spanish Settlers on the Northern frontier of New Spain*. Norman: University of Oklahoma Press. 1979. Pp. xv, 351. \$22.50.

Oakah L. Jones, Jr., approaches the well-plowed field of Spanish borderlands history with a desire to rescue the civilian settler, the common man of New Spain's northward advance, from the relative obscurity to which he has been condemned by many historians. Following in the footsteps of the late Herbert Eugene Bolton, historians have often preferred to concentrate on the deeds of explorers, missionaries, and soldiers. These luminaries appear in Jones's volume but in the unfamiliar position (for them) of background to the unheroic struggle for existence of the *paisano*.

The author divides the vast region of the northern frontier of New Spain into four parts: the northeastern frontier, consisting of Coahuila, Nuevo León, Texas, and Nuevo Santander (Tamaulipas); the north central frontier of Nueva Vizcaya (Durango and Chihuahua) and New Mexico; the northwestern frontier of Sonora and Sinaloa; and the Pacific frontier of Baja and Alta California. He differs from the Boltonians in making Sonora and Sinaloa into a separate area of northward advance, and he gives New Mexico proportionately more space than other single regions. He omits Louisiana and Florida.

The book deals with such matters as occupations and work habits, education, mail service, crime, Indian problems, disease, class differences, and other aspects of the social history of these frontier areas. There gradually emerges from the kaleidoscope of pictures the outline of the stoic figure of the *paisano* and his family occupying the land for Spain despite poverty, isolation, and lack of information.

The author is at pains to contradict the black legend still clinging to Spanish settlers as large landowners who exploited docile Indians. Indians like the Apache and Comanche of northern New Spain were anything but docile, and lack of labor forced the Spanish settlers (most of them Mexicans, as the author points out) to do their own work. Jones frequently emphasizes that these Mexican families on New Spain's frontiers were hard working but, although he cites the Jesuit father Ignaz Pfefferkorn's opposite view of the situation in eighteenth-century Sonora, he makes no comment on it. He does not mention similar criticisms that were made in Alta California, where the mission Indians were later said to be doing much of the manual labor that

kept the provincial government going. Jones also has some remarks of interest on the influence of the northern frontier on Mexican history, apparently following the ideas of Silvio Zavala.

Much of the book is based on material selected from standard primary sources on and secondary literature for the region, but the author has gone to local archives for fresh material on demography and other questions of social history such as prices. The book contains a useful bibliographical essay, six rather roughly drawn maps, and twenty-nine illustrations. The author has tackled his difficult assignment methodically and conscientiously. His work should stimulate further interest in an important subject.

C. ALAN HUTCHINSON
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WILLIAM L. SHERMAN. *Forced Native Labor in Sixteenth-Century Central America*. Lincoln: University of Nebraska Press. 1979. Pp. xvi, 496. \$18.50.

The publishers of this impressive volume rightly contend that the social history of the sixteenth-century conquest of Latin America during the formative decades when class structure was evolving has yet to be written. This book goes a long way to present the first truly comprehensive investigation of the relationship between forced native labor, class structure, and colonial society in that part of the Viceroyalty of New Spain that today is Central America. The volume traces the social, economic, and demographic conquest of early Central America and it details the rupturing of Indian traditions as the Spanish conquistadores and crown officials began the unscrupulous process of exploitation of the only real source of wealth in the sixteenth century, that of native labor. It was in Central America that the Spanish crown made the first serious attempt to reform the labor system in the New Laws of 1542 and to humanize Indian policy. The book details the struggle to improve conditions in face of strong opposition of the settlers.

William L. Sherman's study is an important contribution to the field of Latin American history. Forced native labor as a topic pervades all aspects of Central America in the sixteenth century—social, economic, political, and demographic. The author manipulates a mass of archival materials drawn from Spain, Central America, and Mexico very skillfully, and he writes in a lucid, graceful, and witty style. Although the thrust of the book is on slavery and various other forms of forced labor in sixteenth-century Central America, the work is primarily a social history that treats the consequences of forced labor. As such the study will be of great interest in all areas of Latin American historio-

graphy as well as to anthropologists, sociologists, and ethnohistorians. There is also a lot of early political history in this work—material that is entirely new or else fuzzy or cryptic in other accounts. There is great value in Sherman's biographical detail on the character of the early *audiencia* judges, the governors, and other officials. A chapter on Indian women is unique—a real contribution to social history. The chapter on native nobility is unusually good and contributes vastly to our knowledge.

The great strength of the study is in the extensive use of original manuscripts, most of which have never been used before. Sherman has spent many years on these sources and has hewn his account out of a mountain of archival rock. He has relied primarily on documents dealing with specific local issues rather than abstract Spanish legal codes—thus bridging the gap between theory and practice so evident in other “theoretical” studies of Spanish colonial labor institutions. In my view Sherman's account may well be the definitive study on colonial labor in Central America. What is so impressive in his treatment of the abundant documentation is his reliance on many cases or examples for each of his generalizations. This is sound scholarship based on the most rigorous canons of our craft. Sherman's interpretations are judicious and balanced. He takes neither an Indian nor a Spanish view of labor institutions and Indian policy. In his preface and introduction he lays out the methodological considerations of his study, preferring to let his documents define the boundaries of the work rather than starting with a prefigured model or conceptual framework. As such, Sherman's work falls into the “traditional” category, but it does indeed synthesize and interpret masses of data on sixteenth-century Central America.

The lengthy section of notes (eighty-seven pages), the excellent appendixes on Indian and Spanish population, the full bibliography of manuscript and printed materials, as well as a functional index, testify to the book's importance.

RICHARD E. GREENLEAF
Tulane University

ROGER NORMAN BUCKLEY. *Slaves in Red Coats: The British West India Regiments, 1795–1815*. New Haven: Yale University Press. 1979. Pp. xi, 210. \$17.50.

Desperate military circumstances in the West Indies during the wars of the French Revolution occasioned the creation of black slave regiments in the British army. The planter class in the Caribbean colonies opposed the formation of these regiments, preferring the maintenance of white troops and fearing dreadful consequences from arming black

slaves to defend a slave society. When the planters refused to cooperate in the recruitment of slave troops, partly for economic and partly for sociopolitical reasons, the imperial government proceeded with its plan and purchased raw slave recruits from Africa. In 1807, slave soldiers were manumitted, though they were retained in the ranks on lifetime service. These forces were satisfactorily organized, trained, and disciplined; they were used with effect against foreign enemies and insurgents; and they exhibited no reluctance to police and defend slave society in the British Caribbean. These points form the basis of Roger Norman Buckley's brief history of the West India regiments, 1795–1815.

Buckley is probably correct in asserting that academic historians have neglected military history and that military historians have neglected the Caribbean. His work adds new dimensions to two important historical issues. First, he amends the thesis of D. J. Murray's *The West Indies and the Development of Colonial Government 1801–1834* (1965) by asserting that the decision by the imperial government to garrison the plantations with black slaves was an important episode in the progressive reassertion of metropolitan control over the political destinies of the Caribbean colonies. Second, he argues that Pitt's government made an expedient decision to delay the abolition of the slave trade on grounds that slave shipments from West Africa were a necessary recruitment base for the West India regiments. His evidence on the second point is circumstantial. The decision to recruit black troops from slave ships was taken at the highest level, and in 1806 the government hastened to contract for a large number of slaves before the trade became illegal. But, if it was reasonable to delay abolition until 1807 on the grounds of military expediency, why was it not expedient to delay it longer? Buckley's argument is an important one, and it may shed vital light on a long-standing problem, but his point needs further elaboration and firmer evidence to show that Pitt placed highest priority on recruitment for the West India regiments and that he and his colleagues were convinced that the type of recruitment operations established after abolition, principally at Sierra Leone, would not have sufficed before 1807.

The limitations of Buckley's work derive in part from his failure to afford a comprehensive overview of the military struggle between France and Britain at the turn of the century. There is no full or clear statement of the strategic importance of the West Indies or of the military operations pursued in the Caribbean theater.

Buckley explores the social context of regimental life, disease problems, civil-military relations, and the eventual disbandment of the regiments. The book relies heavily on War Office and Colonial Office archives, and it is generously seasoned with mi-

nor points of considerable interest to imperial and Caribbean historians.

WILLIAM A. GREEN
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SAKARI SARIOLA. *The Puerto Rican Dilemma*. Port Washington, N.Y.: Kennikat Press. 1979. Pp. 200. \$15.00.

This book should not be dismissed as just another rehash of the seemingly ageless *Paradox* (1947), *Plight* (1935), or *Unresolved Problem* (1945) so familiar to the student of Puerto Rico. Sakari Sariola has adorned his description of the Puerto Rican dilemma in very modern terminology and unconventional dialectical analysis. From the outset it must be stated that although the author recognizes the importance of historical perspective, he uses history "as a sociologist focusing on the system qualities of the collectivity at hand . . ." (p. 7). Yet Sariola stresses in the opening paragraph of the preface that his methodology does not follow the conventional pattern: "Instead, I have stressed reactive, willful, and ideology-bound elements" (p. 2).

Thus forewarned, the historian can pass over the erroneous dates (pp. 53, 97, 103), the mistaken names (pp. 94, 104), the extensive use of an 1847 German translation of a French work reissued in 1973, or an occasional error in fact (p. 158). And the social scientist need not worry about objectivity or "an apolitical appraisal." Yet, heretically enough, this is precisely the value of the book, perhaps not for the historian or the conventional social scientist but certainly for the astute and incisive social observer or the dialectical political analyst.

"This book focuses on the search for answers by Puerto Rico's vanguard to the question of political identity and on the compromise solutions they have proposed for implementation" (p. 14). The author brings into sharp focus the multitudinous statements of the intellectual elite of the "independentist" movement ranging from the early revolutionaries of the past century to the current most-wanted members of the clandestine *Fuerzas Armadas de la Liberación Nacional* (FALN). His focus is less sharp, and less generous, when expounding on the also revolutionary and, in the opinion of many Puerto Ricans, brilliant concept of sovereignty contained in the *Estado Libre Asociado* before it was emasculated by a jealous Congress.

The distinction between *Patría* and *Nación* is a basic framework provided by the author for the Anglo-Saxon reader who too often is mystified by the flood of Latin rhetoric. His analysis of the ideas of Pedro Albizu Campos is excellent, revealing their clarity and logic as well as their archaic and prejudiced quirks. Indeed, throughout the book the intellectuals' case for independence is most eloquently

put. For the reader of independentist persuasion one question becomes most pressing as he proceeds to the final pages: "Where is the dilemma?" The dilemma comes at the very end because in the final analysis Sariola cannot ignore the socioeconomic reality that over three million people are being sustained by an incredible outpouring of federal largesse. The author is too much of a realist to suggest that these millions are about to throw out their meal ticket. He does not, however, rule out the possibility of drastic change: "discontinuance through ideological overturning"!

THOMAS MATHEWS
University of Puerto Rico

PETER MARZAHN. *Town in the Empire: Government, Politics, and Society in Seventeenth-Century Popayán*. (Latin American Monographs, number 45.) Austin: Institute of Latin American Studies; distributed by University of Texas Press. 1978. Pp. xxiv, 218. Cloth \$14.95, paper \$5.95.

Peter Marzahl's study of seventeenth-century Popayán is a valuable, though in some ways disappointing, addition to the historical literature on colonial New Granada. On the basis of original research in the archives of Popayán, Seville, and to a lesser extent Quito and Bogotá as well as an examination of the available printed materials, he offers a fully documented account that both describes conditions as they existed during the period and endeavors to analyze and explain at least tentatively the changes that occurred. It is topically organized, with separate chapters on "Estates, Mines, and Commerce," "Cabildo Government," "The Church and the Settlers," and so forth. There are a useful glossary and appendixes; some tables, mostly of names not numbers; and maps, one of which has a disconcerting and quite indefensible mixture of German and English legends.

If from the different chapters one unifying theme emerges, it is that of steady consolidation of local leadership in the hands of a "settler" aristocracy composed of family networks and always open through marriage to new recruits. The same aristocracy receives the bulk of the author's attention, whether he is treating political, economic, or social affairs, and though there are good reasons for such a focus, it naturally does mean that the picture given of life in Popayán is far from complete. In dealing with the aristocracy and with the state and church officials with whom it interacted, moreover, Marzahl devotes perhaps too much space to rehashing the petty squabbles that are conveniently mirrored in *residencia* records and the like. Roughly half of his chapter on the church relates squabbles between governors and bishops—after which he says that

these constituted "the exception, not the rule" (p. 150). If that was the case and if, as Marzahl further suggests, the church was in many ways a stronger institution than the state, such treatment of its role seems less than adequate.

A final problem concerns the relative emphasis on "town" and "empire." In the introduction, Marzahl indicates that he proposes to investigate Popayán both in its "own terms" and "in relation to the empire as a whole" (p. xviii). He successfully brings out a number of special features, including the impact of a shifting mine frontier and the way in which Popayán was subject to the competing powers of attraction and overlapping jurisdictions of Quito and Bogotá. But the insights that he gravely offers concerning the nature of the larger imperial system of which Popayán formed part—or the administration of "the empire as a whole"—seem to add little that is new. For most readers, therefore, a bit more "town" and less "empire" would have been desirable.

DAVID BUSHNELL
University of Florida

JOSÉ HONÓRIO RODRIGUES. *História da história do Brasil*. Volume 1, *Historiografia colonial*. (Brasiliana, number 21.) São Paulo: Companhia Editora Nacional. 1979. Pp. xxii, 534.

This is a book of bio-bibliographical comments and essays (organized under appropriate subject headings) of surpassing interest for the study of Brazilian colonial history. It is not a calendar of "documents" as such but an impressive series of descriptive analyses of the lives and writings of priests and laymen who themselves survived the colonial experience and put pen to paper in their own fashion.

José Honório Rodrigues promises two more volumes to round out his comprehensive survey of Brazilian historiography, one on the national period and one on historiography and ideology. The latest foray into bibliography by a master of the genre will not be the last of his scholarly undertakings, but if it were, nothing would more fittingly end a distinguished career. Bibliography and instruments of scholarship, rather than monographs built upon patient archival research, have been his forte right along, a field of endeavor where his talents, despite outbursts of peevishness, have best been displayed.

If anyone has ever doubted the abundance or fascination of the historical literature of Brazil's Portuguese past, the present array of sources will force him to think again. For the book does indeed do very well what it set out to do. Every student of Brazil and of the overseas expansion of Europe since 1500 will be grateful to the author for his fine over-

view of the history of the history, if not of Portugal's eldest daughter, certainly of its traditional milk cow.

This is not to say that I was pleased with every aspect of the grand compilation. I admired the author's ability to place each item in historical perspective. I liked the tribute that he pays throughout his pages to the memory of his late lamented mentor, Capistrano de Abreu, the most perceptive of the modern historians of colonial Brazil. I liked much less the tendency to make partisan judgments that serve merely as personal statements. His denunciations of the Portuguese are aggressive and abrasive, more to be expected from a self-righteous moralist than from a historian. He is equally at odds with the missionary enterprise, with which he is hardly in sympathy, and upbraids an ancient monarchy that managed to endure precisely because it elicited enormous loyalties from its Brazilian vassals.

There are appalling typographical errors and, what is more serious, altogether too many slips that careful editing would have caught. Rodrigues knows that we cannot transpose the patriarchy of the eighteenth century from Lisbon to Salvador, or identify Oporto as the archdiocese that it never was, or create the diocese of Olinda-Recife before its time. He also knows the risk of using words that lexicographers have thought best to ignore. Above all, his excessive dipping of the pen in bile makes us question his perceptions of the colonial period and wonder whether or not the realities of Baroque structures may best be perceived through populist and "progressive" spectacles.

If one overlooks the exclamations and touchiness, what remains is a superb catalogue of historiographical sources, a much needed bibliographical survey of the colonial period, of history as told by those who lived it, the product, in short, of a man of great reputation who examined everything and now shares his insights with us.

MANOEL CARDOZO
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JOSÉ HONÓRIO RODRIGUES. *O Conselho de Estado: O quinto poder?* Brasília: Centro Gráfico do Senado Federal. 1978. Pp. xv, 417.

The 1824 Constitution of the Empire of Brazil specifically provided for four "powers," or branches, of government: legislative, executive, judicial, and moderating. The moderating power, assigned to the crown, gave the emperor a large measure of control over the other three branches, enabling him to veto legislation, dissolve the Chamber of Deputies, appoint senators, appoint and dismiss cabinet minis-

ters, commute judicial sentences, and pardon convicts. To assist the monarch in wielding the moderating power, the constitution provided for a Council of State, composed of life-term appointees named by the emperor. José Honório Rodrigues, in *Conselho de Estado: O quinto poder?*, contends that the Council of State was, in fact, "the fifth power" of government in imperial Brazil.

According to Rodrigues, there were three councils of state during the imperial period. He argues, unconvincingly, that the first was the *Conselho de Procuradores* convoked by Dom Pedro I at the beginning of his rule over *de facto* independent Brazil. This body, however, was actually a board of provincial representatives and bore little resemblance to the later councils of state. The *Conselho de Procuradores* was suppressed shortly after the convening of a broader representative body, the Constituent Assembly of 1823. The first Brazilian Council of State, denominated as such, was created with the emperor's dissolution of the Constituent Assembly in 1823; this was a ten-man committee that, on Dom Pedro's orders, drafted the document that in 1824 was proclaimed the Constitution of the Empire of Brazil. The Council of State was retained under the constitutional reign of Dom Pedro I as an advisory body that the emperor and his ministers were required to consult on important matters. During the Regency, in 1834, the Council of State was suppressed, but, in 1842, early in the reign of Dom Pedro II, it was reconstituted; it expired with the monarchy in 1889.

Rodrigues's book concentrates on the debates, especially in the imperial Senate, on proposals to create, abolish, or modify the Council of State. Paraphrased parliamentary speeches and long quotations are interspersed with the author's lucid commentary. There is, however, too much repetition of both ideas and rhetoric (for example, one five-line quotation appears in the text twice, pages 172, 176). Although the concept and the legal structure of the Council of State are more than adequately covered, Rodrigues provides little information on what the council actually did, on what went on at its meetings. Only three Council of State sessions are specifically mentioned, one in 1824 and two in 1889, and the author contradicts himself on the consensus of the first (pp. 115, 130). Among the volume's valuable features are biographical sketches of all the imperial Councillors of State. The usefulness of the seventy-two biographies of councillors of the Second Reign, however, is limited by Rodrigues's omission, except in the case of Sales Torres Homem, of any reference to party affiliation.

The composition of the Council of State indicates its power. With a few notable exceptions—including Zacarias de Góis, the baron of Cotegipe, and José Antônio Saraiva—the great statesmen of the

Second Reign were Councillors of State. They, according to Rodrigues, exercised the "fifth power" and were the "guardians of the fourth power," or moderating power. "The fourth and fifth powers," Rodrigues writes, "were ingenious creations of the dominant Brazilian minority," devised "to subjugate the masses," which the elites "feared—and even detested." However admirable the intellectual attainments of its members, the Council of State, Rodrigues insists, "helped enslave the Brazilian people" (p. 281). The matter warrants further investigation.

NEILL MACAULAY
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VERA BLINN REBER. *British Mercantile Houses in Buenos Aires, 1810–1880*. (Harvard Studies in Business History, number 29.) Cambridge: Harvard University Press. 1979. Pp. xi, 206. \$17.50.

Vera Blinn Reber's *British Mercantile Houses in Buenos Aires, 1810–1880* is published as the most recent of the series Harvard Studies in Business History. The focus of the book is business history; Reber is best in her presentation of the operating conditions and practices of important firms in Buenos Aires. Her work draws on extensive research in British and Argentine archives, and Reber has often chosen to let the facts speak for themselves. As a result, she does not discuss at length the works of other authors writing in this field, general theories of business and economic history, or economic development as applied to Argentina.

Background for Reber's treatment of British mercantile houses is provided in the first three chapters of the book: "A Perspective," "Risk and Opportunity in Argentine Trade," and "Politics and Society in Buenos Aires." Chapters four through seven, "The Organization and Operation of the British Mercantile House," "Operating in Argentina," "Merchants, British Investment, and the Economic Development of Argentina," and "British Merchants as Entrepreneurs," provide many helpful examples of the significance of the operation of merchant houses. Initial British success led to many imitators: the British share of import-export houses in Buenos Aires fell from 33 percent in 1825 to 18 percent in 1866 (p. 56). Reber indicates that current problems faced by foreign businessmen had nineteenth-century origins, observing that "the problems created by the Argentine political system could be decreased by forming partnerships with influential nationals" (p. 64). But she also shows that the traditional unlimited liability of partnerships involved considerable risks, since, in the case of failure, "claims were made upon the entire personal estates of all the partners," and bankruptcy of one

partner therefore could cause the remaining partners' other ventures to fail (p. 65).

Occasionally, Reber lets a dramatic opportunity slip by. The dictator Juan Manuel de Rosas insisted that everyone wear his own red colors and the blue and white colors of the revolutionary years were banned. Wearing them thus brought far too great personal risk to make purchase of blue cloth practical. Reber does not mention this ban but merely indicates that "successful marketing of goods depended on . . . the merchants' ability to compete in terms of . . . good will." And she notes that a merchant complained that a "recent consignment of textiles had ignored all advice, as it comprised mainly green and blue colors unsuited to the market for political reasons so that shopkeepers would not touch them. He added that competitors had recently received preferred patterns in red and pink colors and felt that Fielden's shipment would be unsalable unless the prices were reduced" (p. 89).

Reber concludes that "foreigners succeeded when they learned how to operate in the political and social spheres" (p. 144) and that, "although merchants acted in nearly every case from self-interest, the over-all effect on Argentina was positive" (p. 145).

This book is recommended to scholars of business history, as a supplement to H. S. Ferns's basic *Britain and Argentina in the Nineteenth Century* and to L. Randall's *A Comparative Economic History of Latin America, 1500–1914*, vol. 2, *Argentina*.

LAURA RANDALL
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CARL E. SOLBERG. *Oil and Nationalism in Argentina: A History*. Stanford: Stanford University Press. 1979. Pp. xiv, 245. \$15.00.

Carl E. Solberg presents in his second book an effective, traditional "case study" of Argentina's oddly named *Yacimientos Petrolíferos Fiscales* ("State Petroleum Deposits"), better known as YPF. The importance of YPF lies in the fact that it has been a powerful force in Argentine economic, political, and ideological history from 1922 to today and that with its establishment Argentina was the first state, excepting the Soviet Union, to found a vertically integrated, state-owned, petroleum industry.

The 183 pages of text focus predominantly on the years from the first major oil discovery on state land in Argentina, in 1907, to the early 1930s, when the nation underwent a military-political seizure of power and entered a new and continuing era of national destabilization.

The monograph is organized chronologically. Chapter one is interesting because it shows "The Struggle for State-Owned Oil, 1907–1914," which

was not much of a struggle, in fact, because of the minimal interest of the national government in its petroleum lands—a lack of concern that resulted from the deeply rooted, liberal-international economic ideology of the Argentine elite. But the author also examines the opposing views of a few early economic nationalists, who sought to defend and extend the national petroleum lands and production against private investors, domestic and foreign. Of the latter, one of the most important, and the most controversial for many decades, was Jersey Standard (Exxon).

Chapter two analyzes the energy crisis caused by World War I, which coincided with the rise to national political power of President H. Yrigoyen and his middle-class Radical Party. The author skillfully traces the failure of that more liberal leadership to grapple with the petroleum crisis and the emergence of a few military leaders who began to state Argentina's energy problems in terms of economic dependency on foreign powers.

The vigorous Colonel (later General) E. Mosconi emerged during the war and properly plays a large part in the remainder of this narrative. Mosconi became head of YPF in 1922 and gave it the administrative leadership and ideological importance that (at least the latter) still shape its role in Argentina. The labyrinth of petroleum politics is marvelous to observe (when one is permitted) in any context. Non-Latin Americanists will also benefit from Solberg's dissection of the Argentine case, with its objective analysis of the links between multinational corporations and Argentine state and national governments. The establishment of these links had the important side effect of injecting new life into the old federalist-centralist controversy. Solberg also describes astutely such important related issues as the living conditions and strikes of the oil workers and the characteristically polarized positions of Argentine military leaders.

The final chapter is a sketchy review of Argentina's petroleum problems—and some achievements—from the 1930s to 1977. Its limited substantive scope raises the question of why the author chose to restrict his account essentially to the period prior to 1930. Much oil has gone through the pipelines since then, and the most dramatic political confrontations have occurred. Yet those who have worked on Argentine history, in Argentina, know the frustrations of trying to obtain documents. (Exxon also frustrated Solberg.) Perhaps Solberg or someone else can push the work forward some day.

Oil and Nationalism is a fine study in petroleum, politics, and dependency versus nationalism. And, like all Stanford University Press books, it is handsomely presented.

THOMAS F. MCGANN
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GABRIEL GUARDA. *Historia urbana del Reino de Chile*. Santiago: Editorial Andrés Bello. 1978. Pp. 509.

Gabriel Guarda has long been known for his contributions to Chilean urban history. This study, which draws on his earlier work, other secondary material, and research done principally in Spanish and Chilean archives, provides an overview of urban developments from 1535 to 1826 within the confines of the old captaincy-general of Chile. The initial chapters move, after a very brief discussion of the Indian and European backgrounds, from the early Spanish settlements, through the destructive effects of the Indian wars and the subsequent primacy enjoyed by Santiago during the seventeenth century, to the revival and dispersion of urban centers under the Bourbons. The remainder is arranged topically, concentrates on the eighteenth century, and delves into urban Chile's economy, inhabitants, culture, and lifestyles.

Guarda is at his very best when he discusses planning, the physical layout of cities, and urban architecture. He is particularly strong in his descriptions of religious buildings and in analyzing the effects of a defensive posture on the design and character of urban centers. Some readers may be troubled in spots by rather broad statements and inadequate treatment. His assertion, for instance, that Chileans held most of the major colonial public posts (p. 184) glosses over a very complex subject. His discussions of groups such as urban blacks and Indians are not well developed. He might have avoided these and similar difficulties by exploiting notarial records further. The work is, however, an impressive synthesis. Through it, Guarda intended not only to focus on a secondary area in the Spanish Empire and thus to rectify an alleged bias among urban scholars toward New Spain and Peru but also to demonstrate that Chilean towns and cities were vibrant and played an important role in the region's history. He succeeds convincingly in that.

The book is beautifully illustrated. Guarda has pulled together an imposing array of town and city plans, urban maps, sketches and photographs, drawn mainly from depositories in South America and Western Europe, and he includes them along with the text and in an extensive appendix. It is one of the best visual collections on any group of Latin American cities available in print. Surprisingly, there is only one map of Chile with its cities (for 1575) to orient the reader. A special index to settlements, with citations, and a very detailed bibliography of published and unpublished materials, organized by topics, round out the volume. They serve, as does the text, to make this a significant study and an important resource for anyone interested in Spanish American urban history.

KEITH A. DAVIES
Vanderbilt University

BARBARA STALLINGS. *Class Conflict and Economic Development in Chile, 1958–1973*. Stanford: Stanford University Press. 1978. Pp. xviii, 295. \$18.50.

This is an important and interesting contribution to the growing shelf of books seeking to answer the question, “What Happened in Chile?” Barbara Stallings is both a political scientist and economist and approaches the question from a broadly Marxist perspective.

The book starts with a theoretical discussion of “the political economy of development” in which the author underscores the major importance of three elements: the role of different social classes, the part played by the state, and the nature and participation of “the foreign sector.” This is followed by a short discussion of the historical background, in both economic and political terms, of the period with which the author is principally concerned.

Most of the book is taken up with the development performance of the three regimes that spanned the 1958–73 period. These were, “of course, the rightist alliance directed by the National Party, the centrist alliance directed by the Christian Democratic Party, and the leftist alliance directed by the Communist and Socialist parties” (p. 53). Each of these regimes represented a coalition of different class elements and therefore followed different policies. The rightist alliance under President Jorge Alessandri (1958–64) was a coalition of upper- and middle-class elements. The centrist regime (to use the author’s term) of President Eduardo Frei (1964–70) tended more than the other two to cut across the various classes, since “the Christian Democrats were the only real multiclass party in Chile” (p. 55). Finally, the leftist regime of President Salvador Allende (1970–73) was based fundamentally on the working class, particularly the blue-collar workers, although it sought backing from the lower middle class.

The development strategies differed because of the class differences among the backers and leaders of the three regimes. Alessandri’s major emphasis was on investment rather than redistribution, on the role of private enterprise rather than the state, and on providing the widest possible freedom for foreign investment. In sharp contrast, the Allende government put major emphasis on redistribution rather than investment, sought to establish the overwhelming predominance of the state in the economy, and largely squeezed foreign investment out of the picture. The Frei government’s policies incorporated elements of both the Alessandri and Allende approaches. It put more emphasis on redistribution than did Alessandri’s regime, although much less than did Allende’s, while putting very strong emphasis, too, on investment. It fostered a

much greater participation of the state in both mining and industry but continued to lay emphasis on the major role to be played by private entrepreneurs. Finally, although limiting severely the participation of foreign investors in their traditional mining sector, it sought new foreign investment in manufacturing and brought it in as a partner of the state.

The author goes into all of these matters in some detail. She has a chapter each, for instance, on the similarities and differences in both the approaches and results of the three different development strategies. Some readers might be surprised more by the discussion of the similarities than by that of the differences. The author’s conclusion is that all three strategies failed. She poses the question of what alternative is left, and answers that, “by process of elimination, the Chilean case points to a socialist model but of a different type than that practiced by the UP: a socialist model with greater emphasis on investment—and therefore on planning and project implementation—and less on the rapid increase of money income” (p. 237). My biases being different from those of the author, I am dubious about this prescription. Still, this book will lead anyone concerned with Chile to a much better understanding of what happened and led to the catastrophe of September 1973.

The book has certain weaknesses. One the author recognizes: because it concentrates on the urban areas, particularly greater Santiago, it misses much of what went on, particularly in the Frei and Allende periods. Interpretations of events might have been different had the rural areas been included in the discussion. One can quarrel also with the sharpness of class lines the author draws, with the importance she gives to the factions within the Christian Democrats, and particularly with her class analysis of these factions.

This is nonetheless a serious and informative book. It should be widely read.

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MATEO MARTINIC BEROS. *Historia del Estrecho de Magallanes*. Santiago: Editorial Andres Bello. 1977. Pp. 261.

In what he calls “a chronicle of a geographic accident,” Mateo Martinic Beros presents for the first time a full account of the history of the Strait of Magellan. Author of several books on the subject, former intendant of Magallanes, and director of the Institute of Patagonia, Martinic is well versed on the strait and has produced an excellent study.

There are five divisions in the book. The first con-

siders the geology and geography of the strait, after which Martinic moves on to his second division, which covers purely historical matters. There is a brief discussion of possible pre-Magellanic voyages, followed by a detailed account of Magellan's discovery and the subsequent voyages, especially the valuable contributions of Sarmiento de Gamboa. Attention is given to foreign penetration, and the Dutch and English receive their fair share of credit for hydrographic studies. Martinic includes a good cartographic history of the strait, which began, he asserts, with its appearance on an Italian planisphere of 1523, the "Padrón Real de Turín." A third division considers the presence of man in the strait, from the aborigines and Spain's ill-fated efforts to found a colony to Chile's foundation of Punta Arenas in 1849. Efforts at economic development receive adequate treatment.

An extensive discussion of navigation in the strait makes up the fourth part. From the difficult and hazardous voyages of the age of sail Martinic takes his readers to the golden age of steam navigation, 1900 to 1914, which was followed by a period of decline that ended only in 1952 after the discovery of oil. Curiously, he ascribes this decline primarily to the imposition of customs duties and the First World War rather than to the obvious competition from the Panama Canal. There is an interesting section on shipwrecks and accidents from the wreck of the *Sancti Spiritus* in 1526 to that of the supertanker

Medusa, which spilled eight hundred million liters of oil into the strait in 1974.

The fifth division is given over to the rule of the strait. A good Chilean, Martinic makes a strong case for his country's dominion. Beginning with Ladrillero's taking possession in 1558 in the name of the king and the governor of Chile, he traces Chile's claim through the centuries to the diplomacy that led to Argentina's acquiescence in 1881. A one-page final tribute to "The Men of the Strait" concludes the work.

Martinic has written an admirable book. It is well organized, contains excellent maps and illustrations, and is frequently enlivened with such fascinating trivia as descriptions of the bottle, barrel, and large zinc box that served successively as a "post-office" in the strait. There are useful statistical tables on navigation and a valuable bibliography. Three appendixes provide a list of fifty-nine maps of the strait, a list of ships that anchored in the Punta Arenas area from 1843 to 1867, and a three-page list of shipwrecks and accidents that highlights the hazards of the passage. Errors are minimal; perhaps most startling is the statement (p. 50) that Balboa named the ocean "Pacific." For those interested in discovery and exploration, this book fills a long-felt need. It should long remain the definitive work on the subject.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

TO THE EDITOR:

I would like to reply to Joseph F. Kett's review of my book, *American Medical Education: The Formative Years, 1765-1910* (*AHR*, 82 [1977]: 729-30). First, I would comment on Kett's assertion that my book provides a "rehashing of familiar material." I admit that much in the book is not "new," but it is an attempt to provide a synthesis that is unavailable in any other single volume. Other more knowledgeable specialists in the history of medicine have been more generous in their comments. Dr. Lester S. King (in *JAMA*, 236: 2674-75), for instance, stated that the book "will have great value for both the cursory reader, who wants a rapid overview, and the scholar, who will be grateful for the workmanlike presentation of data." Dr. George Rosen (in *Change* [April 1977]: 57-58) likewise indicated that, although the material was not "new," "Kaufman has provided a synthesis that can be used by anyone who wishes to understand how American medicine reached its present state of scientific eminence."

Then, I want to comment on Professor Kett's hypothesis regarding the relationship between economics and the low standards in the nineteenth-century medical schools. Among other things the standards did reflect the economic state within the profession, but Kett's statement is too simplistic to be accepted by anyone with any familiarity with the sources. Other factors included the changing state of medical knowledge, the transition from apprenticeship to more formal training, and the competition among the colleges. Kett really oversimplified in his assertion that I indicated that change came after 1900 because reformers now had "a few allies." When those allies included state li-

censing boards, perhaps they only needed a few allies! A school that refused to improve often found that its graduates could not be licensed to practice medicine, and that was quite an incentive to reform. In reality, however, I indicate in my book that there were other reasons for reform coming after 1900. First, there had been a tremendous increase in medical knowledge; now a modern medical education included extensive use of laboratories and hospitals. It was becoming more difficult and more expensive to provide a medical education. When the licensing agencies insisted on reform, many institutions simply could not afford to remain open for business. This was the death knell of the proprietary medical schools that had been financed solely from student fees.

Finally, if Kett's theory is correct, that standards reflected the meager resources of students and the meager compensation of practitioners, increased standards should have reflected a prior increase in compensation. This was not the case, not until the 1940s and 1950s, as a result of the forces set in motion by the Flexner report. Kett's theory, then, is more of a shot in the dark, and it is not supported by the sources.

MARTIN KAUFMAN
Westfield State College

PROFESSOR KETT REPLIES:

The favorable reviews cited by Martin Kaufman describe his book as workmanlike and useful. My own review called it a careful study. Were Kaufman working in an undeveloped field, a workmanlike summary of existing knowledge might well merit accolades. Were this Kaufman's fledgling effort, a reviewer might well have pulled his punches. But there exists an abundant literature on the field of medical education and Kaufman had already published a book on homeopathy, which I gave a favorable review. I expected more from his second book—and found less.

Professor Kaufman's retort to my review provides additional evidence of the dismaying insularity that

marks his study of medical education. Let one example suffice. Like all others who have studied the history of medical education, Kaufman recognizes the critical importance of the period between 1890 and 1920 and the attendant upgrading of medical standards. Yet in explaining the change he never develops a perspective on reformers different from the one they had on themselves. He ignores the growing literature on professionalization, ignores opportunities to make comparisons to the experiences of other professions during the same period, and ignores political, social, and economic history. When confronted by criticism, he takes two tacks. First, he dismisses economic considerations by noting that only during the 1940s (after the transformation of medical standards) did the annual earnings of physicians surpass those of other professionals. But reference to such aggregate statistics scarcely settles the question of whether certain types of medical education were beginning to pay dividends in the form of superior earnings during the 1890–1920 period. Although this question does not exhaust the range of social and economic considerations that Kaufman neglects, it is significant that he never even asks the question. Second, Kaufman reminds us of the important role played by licensing boards and advances in medical knowledge in the upgrading of standards. In my review, two-thirds of which summarizes Kaufman's thesis, I did justice to these factors. But it is still reasonable to ask how these licensing boards were able to acquire such power so quickly against the vested interests spawned by a half-century of proprietary medical schools. Here again Kaufman neither provides an answer nor poses the question.

JOSEPH F. KETT
University of Virginia

TO THE EDITOR:

My *Hitler among the Germans* may make a good target, but it is beyond Geoffrey Cocks's range (*AHR*, 82 [1977]: 1009–10).

Its thesis is not—doubly not—that Hitler's anti-Semitism and expansionism “were the products of a traumatic reliving of his mother's death.” Its thesis is that Hitler's anti-Semitism resulted from his mother's death itself while his expansionism originated with his German public. Again, that Hitler prompted the iodoform treatment of his mother by Dr. Bloch is no “link” in my argument. My “source” for that prompting was not Kubizek's testimony, but Hitler's own (see my pp. 16–19, *passim*). Nor do I “prove” the traumatic effect on Hitler of his mother's death just by his “phobic references to cancer”: that is artless caricature.

Professor Cocks remarks parenthetically that my “whole argument involving overdosage of iodoform

has been disputed by Bloch's daughter”—close parenthesis. That is disingenuous. Bloch's records show lump-sum charges for daily applications of iodoform gauze. I factored out the iodoform content assuming that Bloch bought 5-meter strips and used about a meter at a time. His daughter contends that a doctor would have discarded the unused remainder of a 5-meter strip. But, first, this contention is false: gauze strips were packaged for use bit by bit. Second, it points nowhere, as Bloch recorded daily applications corresponding to those lump-sum charges. And, third, the iodoform content would come to roughly the same massive overdose even if he bought the gauze in 1-meter strips instead. That is the long and short of the dispute by Bloch's daughter, as Cocks must have known from *History of Childhood Quarterly*, 2 (1974): 265–68.

My concern in *Hitler among the Germans* was “to isolate the few decisive inner demands and constraints” behind Hitler's politics (pp. x–xi). Cocks objects that Hitler's and Germany's “fears” were “overdetermined.” But I was expressly distinguishing determinants from overdeterminants. Further, Cocks cites Peter Loewenberg to fault me for ignoring “intrapsychic fantasy as a major motivating factor in human behavior.” Understandably, Cocks fails to say what Loewenberg meant. This is that the facts of Dr. Bloch's treatment do not matter—that only Hitler's ensuing “intrapsychic fantasy” does. With that, Loewenberg and now Cocks would take the history out of psychohistory.

Professor Cocks approves me for eschewing “the simple application of psychoanalytical concepts to history.” Then sure enough, he himself simply invokes “good” theory to affirm, as against my reading of the Hitler records, that “Hitler's fears” “coursed developmentally from personal to cultural discontents” and the like. It is fun when a pot shot boomerangs like that. But did not my “energetically intelligent contribution” deserve a review in kind?

RUDOLPH BINION
Brandeis University

PROFESSOR COCKS REPLIES:

Rudolph Binion's disavowal of Kubizek's evidence as a link is an indication of the overweening confidence he has in his subjective analysis with its exclusive claim to objective validity. In downgrading Kubizek's “testimony,” he elevates “Hitler's own” as the source for his psychohistorical analysis. Such conviction renders my paraliphtical mention of Mrs. Kren's challenge “disingenuous.”

Beyond establishing that Klara Hitler died from iodoform poisoning, Professor Binion must show that this “trauma” was the “determinative” cause for Hitler's career. Although it is clear that Binion knows more than anyone else about the minutiae

surrounding the events of 1907 and that he can muster a slew of Hitlerian utterances in support of his thesis, the vital issue here is not quantitative but qualitative—that is, the evaluation of the meaning of Hitler's words and deeds. Given the mass of material we have on and from Hitler, it is not surprising that historians have found therein almost whatever they have wished to find. Such is the nature of history. Binion turns vice into virtue. In an essay entitled "Doing Psychohistory" (*Journal of Psychohistory*, 5 [1978]: 313–23), he reported happily, "My own odd experience has been that the key pieces always survive, however little else does" (p. 314).

The almost unrestricted espousal of empathy won through the exhaustive investigation of documents is for Professor Binion the psychohistorian's credo in opposition to what he sees as the ahistorical application of theory and model. The issue raised is a valid and important one, but, and here is the failing in Binion's attitude as I see it, the resultant defense of his work suffers from an absolute conviction that parallels the search within the work itself for certain causes rather than conditions. Thus, Binion's distinction between determinants and overdeterminants begs the crucial question of the certainty with which we may accept his thesis that Hitler's "Bloch complex" in its unwinding and entwining with the German trauma was the cause for his actions. History cannot be replicated and psychohistorical interpretation cannot be verified like a scientific hypothesis. In terms of having his thesis empathically confirmed by other historians, Binion is therefore left with a tautology: "If you see things my way, you'll agree with me." This is one great danger in too great a reliance upon empathy.

Professor Binion himself, in any case, is not free of what he sees as the burden of theory, since his interpretation of Hitler's words and deeds is informed by a variation of orthodox psychoanalysis peculiarly attractive to the historian who seeks strict cause and effect. In this model of historical causation the trauma is an "event" in service to a singular certainty and as such omits the appreciation for multiple causes, process, and change that marks the best historical inquiry. If, however, this model is so compelling, what is it about other models, theories, and cases of human behavior that make them less so? Modern psychoanalysis, more in line with historical inquiry, investigates the total character within a psychosocial life-history rather than reducing symptoms back to a trauma. I have no objection to declaring a central cohering purpose in Hitler's life—his vision of the Jew certainly fits the bill—but the reliance upon a single mechanism whose operation may be detected by a subjective immersion in his words is much too problematic. Binion's lame dismissal of the Soviet autopsy and his hesitant espousal of the testimony of Hitler's physicians re-

garding the number of *Führerhoden* is an example. If Bromberg and Waite, who are the chief proponents of the cryptorchism theory, are right, then Binion's claim to exclusive authority is empty. The limitations upon the use of empathy are internal as well. The historian knows how things are going to turn out. That's at once an advantage—that is, knowing what to look for, but at the same time a circumscription of the ability to become one with the subject aside from whatever psychological baggage the historian brings along. What is crucial is balance: a proper concern for letting the documents speak for themselves, the coincident use of objective and subjective interpretation, and the careful application of theory and case study. The reward is greater: Hitler and the Germans do not just touch at the points of trauma (or does a trauma have edges?) but reflect and reveal each other in a complex of conditions and happenings that does justice, even in its tragic moments, to the richness of history.

GEOFFREY COCKS
Albion College

TO THE EDITOR:

In his review of my book, *The Frankfurt School* (*AHR*, 83 [1978]: 978–79), Paul Breines accuses me of equating sociology with functionalism without giving any proof for his nonsensical statement. His error is somewhat mitigated by the fact that as a historian he does not know the field well enough to make any valid judgment.

I was glad, however, to see that he was unable to come up with any substantive error in my book; I am sure he was searching for it as only a conscientious positivist researcher could. He found my only shaky relation to the facts in the "unfounded assertion" that the Frankfurt School accepted Dimitrov's definition of fascism, a definition that supposedly appears "too often." How often, may the reader ask? Exactly twice. And, since quotations are provided on pages 74–75, the closeness of the definitions of Dimitrov and Horkheimer can easily be compared by the readers of the book. I find therefore that Professor Breines's own "relation to the facts of my book are not always felicitous."

Professor Breines erroneously claims that my notion of Marxism, against which I measure the alleged Marxism of the Frankfurt School, is based on "the hollow assumption that Marxism is what Kautsky or Plekhanov or Zhdanov said." Had Breines really read the book he would have found that its notion of Marxism is based on the theories of Marx, Engels, Lenin, Lukács, Korsch, and Bloch. Should he still harbor some doubts, he could consult the name index and find out that Zhdanov appears only in his imagination and that Kautsky and Plekhanov are mentioned in one footnote.

According to Professor Breines, I saw the diverse writings of Horkheimer and Adorno in the 1930s and 1940s as "contradictory phases" in their careers instead of regarding the essays as "historical components of a unified account of fascism." I wrote about the shifting emphases of the authors; indeed, prior to 1939, their essays had very little to say about fascism specifically. Breines further complains that I ignored the central place of Hegel in the work of the Frankfurt School and its attempt to fuse it with Jewish insistence on the primacy of ethics. I wish he would enlighten us and *demonstrate* the central place of Hegel in the life-work of Horkheimer.

In conclusion: it is gratifying to know that in judging the work of the Frankfurt School I am in the good company of some of the seminal thinkers of European intellectual history such as Lukács, Bloch, Leszek Kolakowski, and Michael Landmann. True, Lukács and Bloch were not "impartial observers" of twentieth-century European history (intellectual and other), but neither is Paul Breines an impartial observer in view of his pronouncements of ten years ago, which he may now term as "youthful aberrations" but which still influence his evaluation of a work (and the result of its analysis) that does not conform to his ideological bent.

I agree with Professor Breines that my book is not a substitute for the books of Jay and Rusconi, but it was never intended to be. I attempted a *sociological analysis* of the *sociological theories* of Horkheimer and Adorno, as I clearly stated on page 10. If Breines is not familiar with the discipline, he should have declined to review the book.

ZOLTAN TAR
City College,
City University of New York

PROFESSOR BREINES REPLIES:

I was honored that in his book, *The Frankfurt School*, Zoltan Tar saw fit to cite a pronouncement of mine from ten years ago. It was part of an enthusiastic look at some of the late Herbert Marcuse's ideas and their impact on young intellectuals in this country. I do not know why he suspects that today I might consider it to have been a youthful aberration. For the record, I stand by it still, as I stand by every line of my brief review of his own book. As to the good intellectual company in which Zoltan Tar believes he is traveling, the claim eludes me. In their published judgments on the Frankfurt School thinkers, Georg Lukács and Ernst Bloch limited themselves to nasty little remarks, while Leszek Kolakowski's reckoning with Marcuse in particular is part of his larger critique of Marx, Engels, Lenin, Lukács, and Marxism *tout court*. I am sorry Zoltan Tar felt compelled to impugn my ability to ap-

praise his book. The review was not *ad hominem*, nor will this rejoinder be.

PAUL BREINES
Boston College

TO THE EDITOR:

In his review of my book, *From Saladin to the Mongols* (*AHR*, 83 [1978]: 1305-06), Michael Dols sharply criticizes two elements of my interpretation of Syro-Egyptian political history in the period 1169-1260. These criticisms are in my judgment not merely erroneous but seriously misleading.

First, Professor Dols rejects my thesis (given on pp. 3-4) that the period under study saw the emergence of a decisive change in the political and administrative relationship of Egypt and Syria. I argue that this change ushered in a long period (from 1260 to 1516) when Syria was embedded within a stable and highly integrated administrative system that subjected it to the direct control of a government resident in Cairo. Dols baldly asserts that this long period represents simply a revival of "the customary relations between Egypt and Syria" and that "Egyptian hegemony over Syria-Palestine" is almost a constant feature of the Islamic era. To this assertion two responses may be made. First, as Dols admits, there was a long hiatus in this traditional pattern—a hiatus extending from 1075 to 1260, or 185 years. One cannot explain the reversal of such a long-term trend by saying that it represents simply a return to "customary relations." Obviously something happened to usher in a new era of Egyptian hegemony, and my book is in part an attempt to identify and analyze this "something." Dols regretably says not a word about this analysis; he does not even allude to its existence. The second response to Dols's assertion is somewhat different. That Egyptian rulers often felt it necessary or desirable to try to dominate Syria I do not dispute, and this point is very plainly made at many places in the book. I do deny, however, that such attempts very often succeeded or that *before 1260* they were ever based on durable, effective, and highly articulated administrative institutions. Anyone who compares the long-lived Mamluk state (1250-1517) with the Tulunid (868-905) and Ikshidid (935-69) polities will see what I mean. Nor is the desperate Fatimid attempt to control Syria between 969 and 1075 in any way comparable to the Mamluk administration in that region. Again, the Mamluk achievement must be related in some way to trends or processes that occurred in the time of its Ayyubid predecessors, and this issue—which is central to the whole book—is not even mentioned by Dols.

Professor Dols's second criticism is aimed at a related thesis, which in fact underlies the one just discussed: that is, a change "in the political goals of the

Ayyubid elite . . . from conscious decentralization to unitary rule." Dols rejects this, saying that "the goal was consistently one of political unity despite the demands of dynastic rule." The whole point of the book is precisely the conflict between decentralization and unity and how this conflict was ultimately resolved in favor of the latter. My argument (which Dols seems not to perceive) can be given in a few sentences. From the 1180s, when Saladin divided his kingdom among his children and kinsmen, down to the death of al-Kamil in 1238, the Ayyubid polity is best understood as a family confederation—as a structure in which political sovereignty was thought of as belonging to the ruling house as a whole and in which each of the more prestigious princes of the ruling house was given a territory to rule as an autonomous principality. The only effective principle of unity was the deference and respect of these territorial princes for the senior member of the ruling house, who was chief of the confederation. On the other hand, this latter figure had to establish some degree of cohesion and common policy, if only to ensure the survival of his house's sovereignty. But insofar as unity was achieved, it was through personal influence, the exploitation of kinship links, diplomacy, or force of arms; down to the death of al-Kamil, there was never any attempt by the head of the confederation to replace the other princes with his own officials—nonmembers of the Ayyubid family who would be dependent solely on him for their rank, status, and power. Only with al-Salih Ayyub (1240–49) was there a serious effort to evolve formal administrative machinery that could support a centralized autocracy, converting former principalities into provinces. With al-Salih Ayyub, we have a clear and definitive change from political decentralization to unitary rule. But this was *not* a change of goal—the goal had always been unity—but a change in the means used to attain that goal. This theme is developed at many points in the book (for example, pp. 10, 62–63, 66–75, 125, 161, 199, 239–240, 283–301); it is unfortunate that Dols overlooked it.

Finally, Professor Dols contests my focus on Damascus, which he believes has the effect of obscuring a political evolution "that was taking place most clearly and significantly in Egypt." Exactly what was occurring in Egypt he does not say, so it is hard to respond to this. I will say that I chose a Syrian focus for my work because it demonstrates that resistance to Cairo's attempts to impose unity was not the work of fractious underlings but of established princes of the ruling house who regarded themselves as rightfully endowed with their possessions and legitimately seeking to define their own interests and establish mutual ties with one another as they saw fit. Dols says that this approach gives

the impression of "interminable warfare" among the princes. Could any other impression be valid when Damascus was subjected to siege eleven times between 1193 and 1260—every time but one by Ayyubid forces?

R. STEPHEN HUMPHREYS
University of Chicago

TO THE EDITOR:

Reviewers have a heavy responsibility. If they fail to exercise their critical faculties, a gullible public may be led astray. But, if they overexert themselves, a serious work of scholarship may be ignored, and their readers may be deprived of intellectual stimulation. It would be a tragedy for all concerned if transatlantic scholars formed their decisive impression of Alfred P. Smyth's important work, *Scandinavian Kings in the British Isles, 850–80*, from Roberta Frank's dismissive review (*AHR*, 84 [1979]: 135–36). In writing this rejoinder, I have no wish to offer a new review or to rebut the (few) points of intellectual substance in Frank's. My own review may one day emerge from the strike-bound presses of the *TLS*, and Smyth is well capable of defending himself. I merely wish to demonstrate that Frank's review is neither accurate nor just (as is indeed hinted by its sneering tone) and to urge readers to discover this for themselves.

The main impression conveyed by Professor Frank is that Smyth is rehearsing familiar and discredited theories about the place of Ragnarr Loðbrók and his sons in Viking activity around the British Isles, without acknowledgement, and without regard for the difficulties raised by others. It is admittedly unfortunate that Smyth had no chance to give more than a cursory glance to R. W. McTurk's careful paper for the *Seventh Viking Congress* (a fact concealed by the delay in his book's publication), and it is true that Smyth prefers to deal directly with the evidence rather than debate it with fellow scholars. Nevertheless, it is quite unfair to imply that he conceals difficulties. To read Frank's review, one would never guess that Smyth *does* make clear that Ari was the first to speak of Ragnarr Loðbrók as "one and the same individual" (pp. 263–65); that he is far from committed to the identification of Ragnarr with the Reginheri who sacked Paris in 845, even if he does not discuss the difficulties here (pp. 31, 98); and that he does indeed attempt to explain how the Ragnarr legend proliferated, and very interesting the explanation is (pp. 36–67, 98–100).

I also share some of Professor Frank's irritation with Dr. Smyth's lack of intellectual generosity in a few cases (notably Professor Byrne, but also Sir Frank Stenton). But there *are* several truly original

insights here—for example, in Smyth's neat removal of Ragnarr from the main line of ninth-century Danish kings, which solves most of McTurk's problems at a stroke (pp. 1–35); in his acute observation of the Viking tendency to attack on church festivals (pp. 148–49, 155–56, 181–82); in his remarkable discussion of the Scandinavian slave trade (pp. 154–68). More important, those of Smyth's suggestions that have been made before have never, as Byrne would be the first to admit, been put so fully. It is precisely because of this width of reference that the whole edifice does *not* collapse, “like a house of cards if anyone is churlish enough to sneeze.” For example, Byrne's few crisp sentences had already virtually disposed of Mrs. Chadwick's objections to the identification of Inwaer in the English sources (where he is not a “Danish chieftain” but one of two leaders of the 865 *micel here*) with Imhar in the Irish (where he is not “one of the leaders of Norwegian Dublin” but *rex Nordmannorum totius Hiberniae et Britanniae*). But it is Smyth's detailed exposition here and in his work on Ivarr's descendants at Dublin and York that has put it beyond all reasonable doubt. (Incidentally, the most important evidence for Inwaer's leadership of the Great Army is not the *Anglo-Saxon Chronicle* but, as Smyth failed to note, the Life of Archbishop Oswald, grandson of a member of the army; this *Vita* was written at Ramsey Abbey, the residence 985–87 of Abbo, whose *Passion of St. Edmund* was arguably the source of all subsequent English knowledge of Ivarr's role.) If, from historical sources alone, the founder of the Danelaw can be shown to have been the King of Dublin, then quite regardless of the sagas the *onus probandi* is shifted onto those, like Frank, who would doubt Smyth's other identifications chiefly on the grounds that the wide-ranging Viking activities involved were absurd *per se*.

I do not say that Dr. Smyth is always right, or that there are not serious difficulties involved in some of his ideas. But it is extremely important that his views are carefully analyzed and discussed by those, like Professor Frank, with the Old Norse expertise to do so, and it will *not* do to misrepresent or dismiss them out of hand. I would add that I write as a friend of Smyth's without any special qualification other than that of being a historian of early medieval England and Europe. But, lest it be thought that this discredits my intervention, I am authorized to say that these are also the views of Peter Sawyer, the leading English expert on the Viking Age and the first to put powerful arguments against sanguine use of saga evidence. *Caveat lector* is a tag more appropriately applied to Professor Frank's review than to Dr. Smyth's book.

C. PATRICK WORMALD
University of Glasgow

PROFESSOR FRANK REPLIES

C. Patrick Wormald says he shares my concern with some of Alfred P. Smyth's scholarly procedures. He mostly scolds me for my sharp tongue and pretends to demonstrate that my review is neither accurate nor just. I dared to complain, for example, that the author tries to spare his readers facts that seem to confuse or contradict his account. The points of rebuttal raised by Wormald illustrate rather than undercut my complaint. He confirms that Smyth waits until the final three pages of his narrative to acknowledge that the names Ragnarr and Loðbrók are not found together until the twelfth century and to admit that the historicity of this legendary figure is “difficult to establish.” I find so late a retreat disquieting in a book that from page one assumes the historical existence of a single Ragnarr Loðbrók. I am glad to learn, on Wormald's say-so, that the author is far from committed to the identification of Ragnarr with the Reginheri who sacked Paris in 845 (the contemporary *Annales Xantenses* reports that the Viking leader died soon after the raid, thereby making it inconvenient for him to visit England around 850); why, then, if it is unnecessary for his argument, does Smyth insist (pp. 31, 98) that the identification of Ragnarr and Reginheri is “plausible,” “probable,” and “chronologically possible”? This concealing of difficulties (to use Wormald's phrase) may be part of a larger problem: Smyth seems to have a theatrical flair for highlighting whatever late or inconclusive evidence is available and dispatching good evidence to the wings (a kind of Gresham's law of scholarly hoarding). Wormald tacitly comments on this tendency when he introduces his own proof for Inwaer's leadership of the Great Army—from a source, he notes, not mentioned by Smyth.

As for the remaining factual objection raised by Dr. Wormald, I am quite aware that Dr. Smyth attempts to outline the early development and transmission of the Ragnarr legend. My criticism, which the letter misrepresents, is twofold: the author did not try hard enough to distinguish chronologically the various layers of source material at his disposal, nor was he able to explain to my satisfaction the strong European interest in the legend around 1200. Unlike Wormald, I am not as worried about Smyth's “lack of intellectual generosity” as I am about his revival of a haphazard eclecticism that was long ago made obsolete by the advent of systematic source criticism.

Dr. Wormald lists what he considers to be three original insights of Dr. Smyth's book. We all have our favorite nuggets; his do not strike me as glistening so brightly as all that. For example, the chapter on slave trading makes much of the shipment of captives from the British Isles to the Islamic west

and even Byzantium, while failing to find contemporary evidence that a single such shipment was ever made. Some may applaud Smyth's chutzpah and call it "remarkable"; others, like myself, will prefer proof to innuendo and consider Smyth's account unwholesome and misleading. I also differ from Wormald on the utility of the author's "width of reference" (my review says "to flesh out"); the chief result, as I see it, is an entertaining narrative that borders on the sensational. No one reading my colorful description of Smyth's book could possibly conclude that the latter is lacking in interest or stimulation.

The final sentence of Dr. Wormald's third paragraph has me genuinely puzzled. All serious students of the period recognize the wide-ranging, international character of ninth- and tenth-century Viking enterprises; I do not doubt Smyth's identifications on the grounds of Viking immobility, and I cannot believe that this is what Wormald really means to insinuate. Is he saying that an acceptance of the Inwaer/Imhar link compels one to approve automatically Smyth's subsequent identifications (for example, Óláfr inn Hvíti/Óláfr Geirstaðaálfr; Caittil Find/Ketil Flatnef) and that anyone who balks has the burden of proving that they are not one and the same? I hope not.

It is too bad that Dr. Wormald's own review of Dr. Smyth's book has been delayed. Nevertheless, I believe that he underestimates the sophistication of transatlantic medievalists if he thinks that they will form their "decisive impression" of Smyth's book from a review limited to 500 words (shorter by some 250 words than his own letter). Dr. Smyth's friends have no reason to fear that his contributions (and theirs) are unknown in North America and that his work will be ignored. Smyth's previous book, which roused high expectations for the current volume, has received long and thoughtful appraisals in specialized journals. R. W. McTurk's careful, four-page review in the *Saga-Book of the Viking Society* (1977) is particularly impressive. Writers of bland books get bland notices; studies as ambitious and provocative as Smyth's run other risks.

ROBERTA FRANK
University of Toronto

TO THE EDITOR:

Nagged by the suspicion that he gave it no more than a casual perusal, I take little delight in Sheldon Harris's grudging admission that my book, *The Golden Age of Black Nationalism, 1850-1925*, is "well-researched, well-written, and most interesting" (*AHR*, 84 [1979]: 262. I wish that he had troubled himself to quote me accurately and that he had discussed the contents of my book rather than just the title.

Professor Harris states the obvious when he observes that "the period under study, 1850-1925, would hardly seem to be a 'golden age' for black Americans, no matter what their pan-African views, . . . and even the enthusiastic author of the blurb on *Golden Age's* book jacket concedes the period 'would hardly seem to mark a golden age.'" These words indeed appear in the publisher's blurb, but they are taken almost verbatim from the second sentence of my preface (p. 9). Far from being the concession of an enthusiastic publicist, they mark my own awareness that a golden age in a people's literary or intellectual history may coincide with a dismal period in their social history. J. A. Hobson, Hans Kohn, Elie Kedourie, and others have long recognized that the flourishing of nationalist ideology often coincides with periods of tribulation. I am amazed that anyone finds this a controversial or troublesome idea.

I am even more amazed that Professor Harris refers to Alexander Crummell as an obscure figure. Certainly he has not forgotten August Meier's observation in *Negro Thought in America, 1880-1915* that Crummell is "generally regarded as the leading nineteenth century Negro intellectual" (p. 42). Although Theodore Draper displayed unconscionable ignorance of him in *The Rediscovery of Black Nationalism*, historians like W. E. B. DuBois, Benjamin Brawley, Otey Scruggs, and Hollis Lynch have long recognized the magnitude of Crummell's contributions.

I cannot respond to Professor Harris's attack on my definition of nationalism, for he does not state his objection. He simply asserts that my definition is "murky" and "frequently contradictory." Perhaps he simply missed my point that classical black nationalism, from Delany to Garvey, was indeed murky and contradictory. In this respect it was no different from other nineteenth-century nationalisms. Whatever Harris meant, he would have had ample room for clarification, if he had not chosen to quote me for twenty-six lines of a seventy-one line review. Under the circumstances, I might have been relieved that the reviewer allowed me to speak for myself, but Harris misquotes me at the beginning of his second column, where he mars my prose with an illogically constructed sentence. I make enough mistakes of my own without taking the blame for his faulty parallelism.

As for the assertion that my book is a collection of essays, I fear that the thematic unity of my chapters and the process of change that they describe eluded Professor Harris. He seems unaware that I am using black nationalism as a vehicle for discussing more general problems in nineteenth-century intellectual history, for example, the confusion of the concepts of culture and civilization. Among other evidences of carelessness and superficiality, there is the failure

to include all publication details. The book is 345 pages long, which information he should have realized was missing when he saw the proofs of his review.

WILSON J. MOSES
Cambridge, England

idea what it weighs. But as professional historians we should be more concerned with the quality of a study rather than its weight or length.

SHELDON H. HARRIS
California State University,
Northridge

PROFESSOR HARRIS REPLIES:

I would hope that Wilson J. Moses is a better historian than he is a seer. As with any responsible reviewer, I read his book quite carefully. That I was less impressed with his study than he is is unfortunately true. My review, however, is balanced and fair. I find no reason for apology.

As for specific points in Professor Moses' unhappy lament: (1) I do quarrel with the appropriateness of his title. Even accepting his thesis concerning a golden literary age with a dismal period of social history, his study covers the wrong period. 1850–1925 was a dismal era for all black Americans, and to call any aspect of their experience "golden" reveals an egregious lack of sensitivity on the part of the person making such an assertion. Should we expect someone to write a sequel to Moses' study and entitle it "The Golden Age of Racism, 1850–1925"? (2) With all due respect to August Meier, Alexander Crummell was not a familiar household name in the period under review, nor is he today. (3) I did not "miss" the point that "classical black nationalism, from Delany to Garvey, was indeed murky and contradictory." Moses never really defines black nationalism. Anyone and everyone is thrown into a kettle labeled "Black Nationalism." Surely, he is familiar with Professor Redkey's study as well as Professor Rudwick's and Bracey's works. And why begin with Delany? Paul Cuffe and other Afro-Americans of an earlier era contributed to a definition of black nationalism. Moses is indeed "murky" in his definition and use of the term "Black Nationalism." (4) I did quote extensively from *Golden Age*. The purpose of the quotations was to inform the reader of what the author was trying to say. Since Moses is unhappy with my written comments, he should be grateful that I devoted so much of my review to direct quotation. Misquotation is in the eye of the beholder. I fear that the "illogically constructed sentence" that Moses refers to is only a reflection of his own writing style. (5) Moses believes he has written a book that has thematic unity. I do not. He has written a series of essays, some of great value, that are held together solely by a very loose use of the term "Black Nationalism." (6) Moses' complaint concerning omission of publication details should be lodged with the editor of this journal and not with me. I am not responsible for the *AHR*'s review format. I should add, however, that I did not place his tome on a Toledo scale. I have no

TO THE EDITOR:

Renate Bridenthal's review of my book *Frauenarbeit im "Dritten Reich"* (*AHR*, 84 [1979]: 485) makes it difficult for me to recognize my own arguments. I do not want to list the many inaccuracies and distortions in what the reviewer pretends to be a reproduction of my conclusions. There is, however, a major omission. Bridenthal does not mention at all that my book disproves the influential interpretation that attributes to the Third Reich a modernizing or even revolutionary impact on German society. Even more questionable is Bridenthal's dealing with what I have written about the role of Hitler.

My critic maintains that I come "dangerously close to apologia" with my conclusion that Hitler's decision concerning the mobilization of women was based mainly upon irrational ideological reasons and that his prohibition of a draft law for women in wartime was not a sign of political opportunism. Bridenthal denies the possibility that Hitler believed in his own ideology. She describes Hitler and the other party leaders as mere opportunists. Bridenthal seems to subscribe to the vulgar Marxist assumption that Hitler and the Nazis were nothing else than agents of capitalism and considered their ideology exclusively as an instrument destined to dominate the masses and to disguise their real purposes—namely, the defense of bourgeois interests. If this were true, how could we explain the utmost atrocities of the Nazis such as the killing of six million Jews?

Those who deny the destructive potential of irrational forces will always be inclined to push them aside as "dysfunctional" or "theoretically unimportant." The inevitable result of this type of interpretation is that a regime such as the Third Reich looks much less cruel than it was in reality.

DÖRTE WINKLER
Stegen-Eschbach,
West Germany

PROFESSOR BRIDENTHAL REPLIES:

I can best respond to Dörte Winkler's letter, point by point, by referring back to my original review.

First, I would like to repeat from it that Dörte Winkler's research is thorough and her scholarship strong, though I take issue with some of her con-

clusions. Second, I acknowledged that Winkler showed the Nazis' stress on the traditional sector of the economy *retarded* modernization, but surely we both agree that they did not cause capitalist development either to stagnate or regress but allowed it to continue. Third, the taint of apologia for Hitler appears to have been very unsettling, so I would like to point out that in the review I added the phrase "surely not intended by Winkler."

Fourth, I did not dismiss Hitler as a "mere" opportunist or, even more mechanically, as an "agent" of capitalism. Rather, I described him as "the 'little man' incarnate, of the bourgeoisie, especially the petit bourgeoisie." That means he was not simply a tool or a puppet, but a true representative, literally, of his class. The sincerity, however real, of his and of its belief in their ideology matters less than the fact that it was self-serving—that is, political. Sincerity usually remains opaque, among the dead even more than among the living, and concerns the historian less than does the more telling question: "A cui bono?" In the case of women, the fact remains that bourgeois women evaded war work and accepted Nazi "protection" of their "womanly role." Working-class women were not spared the rigors of patriotism.

Fifth, mentioning the atrocities seems a non-sequitur in this context, except as an anti-Marxist aside, so I will address it as such. Marxists do not dismiss irrationality but consider it as being at the core of capitalist dynamics. Thus, irrationality is not dysfunctional; it is one of the meanings of the term "contradiction" in a dialectical movement. Scapegoating—of Jews, blacks, women, and the like—is certainly not theoretically unimportant and is usually quite functional as a diversion from social division by class. An aside of my own: the automatic gluing of the word "vulgar" to the word "Marxist" is itself vulgar and a time-worn cliché. By now, even an *ad femina* insult would be preferable.

RENAE BRIDENTHAL
Brooklyn College and
The Institute for Research in History

TO THE EDITOR:

Alexander Orbach's review of my book, *The Jewish Intelligentsia and Russian Marxism* (*AHR*, 84 [1979]: 504-05), notes two problems with the work. These appear to be sufficiently serious to warrant a reply.

The stated purpose of the book is to begin constructing a structural theory of intellectual radicalism and ideological divergence through the use of qualitative and quantitative data on over two hundred Jewish members of the Bolshevik, Menshevik, Bundist, and Poalei-Zionist parties in turn-of-the-century Russia. In the course of my research I came

to the conclusion that a substantial portion of the variance in the ideological views of the persons in my sample can be explained by examining the paths of social mobility that they traversed in the course of their careers: they tended to learn culture patterns associated with the different positions that they occupied in society at various times, and this learning predisposed them to join one party rather than another.

Professor Orbach questions the usefulness of my theoretical approach on the grounds that it is "highly restricted in its application": my explanation may work well for intellectuals in the four parties I have chosen to examine, but not, apparently, for anarchists, populists, and the like. One problem with this criticism is that it may or may not be valid. We do not know since no comparable data are as yet available on anarchists. More important, perhaps, it suggests a quite peculiar attitude to the role of theory in sociohistorical research: rather than trying to reformulate theories that work better than others, so that workable theories can accommodate a still broader range of data, Orbach suggests that we reject entirely the workable theories if any apparently contradictory evidence can be presented. On this advice, we could hardly expect any progress at all in the realm of sociological theory construction.

Professor Orbach also states that my theory is marred by an internal inconsistency: if, as I sought to demonstrate, the social origins of Poalei-Zionists differed very substantially from the social origins of Bolsheviks, how can one explain the fact that the two groups of intellectuals came closest to each other on certain doctrinal issues? The answer has to do with certain similarities in what I refer to as their "social destination"—that is, the classes and strata of Russian society to which they were, because of their social circumstances, more or less compelled to attach themselves when they entered adult political life. I discussed this point at considerable length in the book, and I cannot understand how it managed to escape Orbach's attention.

ROBERT J. BRYM
University of Toronto

TO THE EDITOR:

I would like to comment on John Burnham's review of Nathan Reingold's *Papers of Joseph Henry*, vol. 2 (*AHR*, 84 [1979]: 547-48), because it has raised important issues for those of us (like myself) who are currently engaged in preparing selected editions of the papers of scientists or physicians.

Professor Burnham makes three essential points in his review. First, he maintains that a selected edition of papers, such as the Henry Papers, does not satisfy the needs of the "true scholar," who needs all

of the papers of a given collection before him. Second, he argues that many of the documents selected by Dr. Reingold and his associates are trivial. And, third, he claims Dr. Reingold's annotations are in large measure exercises in antiquarianism. Burnham's assertions raise all sorts of questions. What should the criteria be for selecting and editing documents? Should the annotations of such documents be detailed? And, finally, what is the ultimate usefulness of such selected editions?

I believe that projects like the Henry Papers are not only important and useful; they are, in fact, necessary. The history of science and medicine in this country are relatively young disciplines. Many of the manuscript collections that inform these disciplines are still in the process of being collected and organized. In recent years, many historians who work in related fields of intellectual, social, and urban history have increasingly found materials from the history of science and medicine useful in reinterpreting our history. They, as well as those historians who are currently being trained in the history of medicine and science, need well-edited, selected documentary collections in the history of medicine and science, easily at hand, to add to their store of information or on which to sharpen their thought and understanding. As a historian of medicine, I would give my eye teeth to have the entire corpus of Benjamin Rush papers before me. That wish, however, does not impair the usefulness or value of Lyman Butterfield's selected edition of the Rush papers. I have used that edition for well over a quarter of a century to introduce my students to problems in the history of medicine. I cannot begin to detail the wealth of information and ideas that edition has given me.

Professor Burnham claims that many of the documents Dr. Reingold and his associates chose for volume 2 are trivial. Trivial in relation to what—the other documents in the Henry collection or innately trivial? Are not most documents trivial until they are given meaning by the imagination and understanding of the historian? There is no faulting the historical acumen and imagination of Reingold and his associates. The documents they have chosen not only illustrate the process of Henry's research, they inform as well the professionalization of science in the United States and family, political, and intellectual history.

Professor Burnham complains that many of Dr. Reingold's annotations are obvious and tell him more than he really wants to know, or will ever need to know, "except in the most bizarre circumstances." Burnham's complaint is more revealing of Burnham than of Reingold. I found the reverse to be true. For me, the annotations were worth the price of admission to the volume. For example, while it is true that Reingold's annotation of the

document, *Henry's Galvanic Experiments on the Murderer Le Blanc*, were longer than the document itself, it illustrated in the most extraordinary fashion the impact of the work of Dr. P. M. Roget (of Roget's *Thesaurus*) on the subsequent development of Henry's thought and research. As a historian of medicine, I further appreciate the rich biographical detail that Reingold and his associates supplied about physicians who lived in the Albany and New York areas and, in particular, about their relationships to the rest of the contemporary intellectual community in the United States.

I would like to add here that Dr. Reingold's annotations have one other important virtue: they stand as guides both to the literature of science in Joseph Henry's day and to the important secondary literature in the history of science in our own time. Professor Burnham believes otherwise. He complains that Reingold and his associates, for all of their scholarship, have overlooked George Daniels's pioneer study, *American Science in the Age of Jackson*. While Burnham does not say it outright, he suggests that Daniels's study was overlooked for some malign personal reason. It is true that Daniels's study is not cited. Still, the study in question was cited several times in volume 1. In addition, Daniels's edited volume, *Nineteenth Century American Science: A Reappraisal*, is cited in volume 2. One can only assume that Daniels's earlier study was not cited in volume 2 because it did not serve to explain the documents that were chosen.

Dr. Reingold's edition of the Henry papers might well serve as a model in preparing selected editions of the papers of scientists and physicians. No matter what the model, we need to increase well-edited, selected editions of such manuscripts if we are to have truly new historical interpretations of our history.

SAUL BENISON

University of Cincinnati

PROFESSOR BURNHAM REPLIES:

The allocation of resources and the use of the scholarly community to facilitate a researcher's communicating his findings are matters about which there may be honest differences of opinion. I am grateful for the opportunity to try to summarize one point of view.

The *Papers of Joseph Henry* provide an excellent example of painstaking scholarship that is not channeled so as to achieve optimum benefit. Much of the effort that went into selection and annotation could have produced instead some excellent articles and notes that would have been available in refereed and indexed journals. As it is, little gems and some important essays are buried in a relatively inaccessible place and cannot be located by any index or similar bibliographic aid (and this is particularly

hard on researchers from neighboring subjects or colleagues dependent on small libraries). The very erudite among us may be able to remember each such discrete item or take notes with elaborate cross references. But most of us will miss benefiting from some of the editors' work because it is not indexed or even placed in a volume or article with a title that describes clearly the major subjects discussed therein.

Saul Benison's elaborate defense of the omission of references to Daniels's standard monograph bears out my point. Had any of the important essays embodied in the footnotes of this volume been submitted to a good journal, the journal editor and his referees would have made sure that the author related the essay to the important literature in the field, thus doing the author the favor of increasing the significance of his work and the reader the favor of showing how scholarship is cumulative.

The question of trivial and antiquarian material presents the temptation to win debater's points by using ridicule—that is, citing amusing and outrageous examples from the volume in question. I would prefer simply to say that once again editors and colleagues can provide reasonable compromise concerning such matters. As scholars we continually discriminate and make judgments. I am glad that we do, and I wish that the published Henry papers showed more evidence of the editor's sense of proportion (and that of his advisors and sponsors) and less of assiduous accumulation.

I believe that money spent on such projects as the Henry papers could be better spent on microfilm editions of collections and on research support for articles, monographs, and books of broad synthesis and interpretation. Comments by some reviewers of similar volumes of great men's papers suggest that I am not alone in these sentiments.

JOHN C. BURNHAM
Ohio State University

TO THE EDITOR:

Alexander De Grand's review of my *Fascism in the Contemporary World* (*AHR*, 84 [1979]: 714–15) contains an unusually large number of incorrect and misleading statements. For brevity's sake, I will rejoin only to five.

First, Professor De Grand believes the "curious" thesis that many authoritarian regimes in late-modernizing societies possess marked similarities to fascism to be my own invention. This is very flattering, but he will find this thesis advanced by A. J. Gregor, A. F. K. Organski, and John Weiss, all cited in my bibliography and acknowledged in the preface. De Grand could also have found Gregor's version of the thesis in the foreword. Second, De Grand seems scandalized at my characterization of Mussolini's regime as developmental. But the same scandalous

characterization appears in the works of A. J. Gregor, Ernst Nolte, Roland Sarti, Domenico Settembrini, Eugen Weber, and many others. Third, De Grand finds my view of the Italian Socialists' responsibility for Mussolini's rise to power the result of a "simplistic and tendentious rendition of Italian history." He might have informed the reader that my position is shared by Denis Mack Smith, H. Stuart Hughes, Maurice Neufeld, Angelo Tasca, and others (see pp. 38–39). Fourth, De Grand says that Vargas was not "brought into this conceptual framework" of Third World fascism, but in fact Vargas's nationalism, corporatism and authoritarianism, and his resemblances to Perón are noted on page 185. Fifth, De Grand states that I did not use "non-English sources in anything but translation." Now how does he presume to know what sources I used? All he knows is that my bibliography is *explicitly* limited to English-language books, with advice to readers of other languages about where to find non-English works on fascism.

Having shown himself hardly more familiar with my book than with the literature on which it is based, De Grand then reveals his notion of the bounds of legitimate scholarly controversy by actually stating that my book "verges on fascist apologetics." I am not sure what good purpose is served by having a book reviewed in a professional journal in the style of the popular pamphleteer.

ANTHONY JAMES JOES
Saint Joseph's College,
Philadelphia

PROFESSOR DE GRAND REPLIES:

Anthony James Joes feels that I have misrepresented his thesis and underestimated his research. My review, however, was directed at the simplistic and mechanical application of the thesis and, more particularly, at his attempt to establish a connection between Italian fascism and a grab bag of societies in different historical contexts and at vastly different levels of development.

I could hardly be unaware of the origins of Professor Joes's ideas, but the sweeping application of the "fascism as modernization" thesis makes it lose all subtlety and much of its validity. The only connection between Joes's interpretation of Mussolini (point two) and the work of Nolte, Sarti, *et alia* is that a rotten and a ripe apple are both apples.

It is true that the Socialist party has been singled out for special criticism by several of the authors mentioned in point three. Again, what I objected to was Professor Joes's tendency to heap so much of the responsibility on the left that it, rather than fascism, becomes the villain. Underlying his whole section on Italy was a justification of fascism as a positive response to the Italian crisis of 1919–20. Largely ignored were the tragic dimensions of class cleavage

in Italy and the destructive nature of the fascist response. I feel that Joes's underestimation of the negative side of fascism until he discusses events after 1936 stems from the abstraction of the model that so universalizes the concept in fuzzy notions of modernization that any sense of its real impact on Italy is lost.

Finally, I am amazed that he should ask how I presume to know what sources he used. Any reviewer must use the notes and bibliography. Professor Joes cleverly hides his non-English-language sources.

As for who is a professional pamphleteer, I leave it to readers of the book to determine whether its violent anticommunism does not fall into that category.

ALEXANDER DE GRAND
Roosevelt University

TO THE EDITOR:

Sally Marks gives the impression of "hardly knowing where to begin" in recounting the failings of *France's Rhineland Diplomacy* (AHR, 84 [1979]: 770-71). I scarcely know where to begin a rebuttal to so undisciplined an assault. No long book handling many sources and a complex subject is without minor errors; what makes the Marks "review" so malicious is that detection of alleged errors was apparently her sole purpose. And, for someone so intolerant of imperfections, she is remarkably imprecise. She has removed statements from context, ignored or distorted nuances, and employed exaggeration and outright falsification in what can only be termed a "hatchet job."

Many of Professor Marks's alleged "errors" are brazen inventions. (1) Never do I bluntly assert that Belgian minister Jaspar "favored Rhenish separatism." Rather, I stress the great nervousness of the Belgian government about illegal separatism, and I show the Theunis-Jaspar cabinet's actions in 1923 to be not "support for separatism" but wily use of a putsch in the Belgian zone to abort French separatist intrigues (pp. 196, 237-38, 256-57, 268, 311-16). (2) Never do I assert that Lloyd George preferred Poincaré to Briand. This is a false allegation based on my accurate statement that at one point the British cabinet aired the notion that, "if M. Poincaré returned to power, it might be possible to come to some more satisfactory settlement with him than with M. Briand" (p. 179). Earlier, Lloyd George was anxious to preserve Briand in power, as I show (p. 147). (3) In my book, according to Marks, "In 1923 Foster Dulles becomes an official American spokesman." My text, however, describes Dulles as "an unofficial envoy" (p. 288).

Other charges are so vague as to be unanswerable. (4) My interpretation, which is never described, is said to be "at variance with historical

data." Never is this demonstrated. (5) I am accused of "a multitude of mistakes." In fact, even Professor Marks's hand-picked few are dubious, wrong, or too trivial to support her grave charges. (6) It is claimed that "more reference to conference minutes" would correct my account of reparations, and (7) that more work in London would have "reduced the level of error regarding reparations, etc." Apparently, Marks is a devotee of the "what one clerk said to another" school of diplomatic history, for many of her criticisms stem from this bias. I, too, believe that diplomatic exchanges are often central to the course of international relations. But where I deem "conference minutes" not of primary import, I believe it my duty to spare the reader. My book was not designed to be another reparations chronicle. (8) Had Marks deigned to show how I "misrepresented" the December 1921 and September 1923 talks, perhaps I could respond to her objections.

(9) Professor Marks's most serious charge—that "evidence is distorted or suppressed"—is also a flimsy contrivance. In the Nothomb affair, Marks is apparently referring to a telegram cited on page 197 (here, as always, it is left to us to guess). Rechecking the document I see that I did inadvertently broaden a report of Jaspar's reluctance to say whether Belgian authorities would support the separatist Smeets in a lawsuit, to reluctance to take a position on Nothomb-style separatism in general. This error is seriously advanced by Marks as "suppression of evidence," even though the issue—and all Belgian matters of such keen interest to Marks—are peripheral to my main arguments. Furthermore, the datum she accuses me of "suppressing"—a repudiation of Nothomb's goals—actually supports my true characterization of Belgian policy as hostile to illegal separatism and any Rhenish schemes unsupported by Britain. (10) Marks's other instance of "distortion" is "an August 1923 Poincaré telegram." Assuming she means the one cited on page 300, her objection escapes me—the quotation in the text speaks for itself. In any case, such potshots are abuses of the reviewer's advantage over the reader. How are we supposed to know just what she is disputing and whether her complaints are justified? That such vagaries are meant to support accusations of extreme gravity seems most unprofessional.

The blanket charge that I "contradict and disprove myself" also reflects Professor Marks's overzealous search for ammunition. (11) A daft nonentity like Dorten was "suited to lead" a motley movement of nonentities. I describe his antics at length (pp. 46-47, 70-72, 122-28, 158-61, 195-99, 280-86, and most of chap. 9)—only the most inattentive reader could think that I sought to praise Dorten's leadership. (12) The charge of Briand's being "for and against" a Ruhr occupation is unsupported and ignores the fact that most French

politicians *were* ambiguous about such a grave, uncertain act. (13) French Rhenish policies *did* involve many competing ends and means that varied over time. French confusion and hesitancy is one of my main themes (pp. 8–14, 244–49, 259–69, 360–66). These are not instances of “contradictions” but of distortion or misunderstanding on the part of the reviewer. With more space, the list would continue.

A book review is supposed to be informative, especially if it is negative. Yet the reader knows no more about my book at the end of Professor Marks’s piece than at the beginning. She ignores the book’s interpretations and conclusions, many of which one would have expected her to endorse. Her “review” is not an attempt to dissuade readers from views she finds untenable; it is an attempt to prevent readers from discovering my views at all! One wonders why. Surely as a serious scholar working in her field, in which there is presumably room for others, I deserve the same courtesy and reflection that Marks would demand for herself.

WALTER A. MCDUGALL
University of California,
Berkeley

TO THE EDITOR:

Sally Marks’s review of Walter A. McDougall’s *France’s Rhineland Diplomacy, 1919–1924*, which appeared in the *AHR* last June, accused McDougall of gross incompetence as an historian, if not outright dishonesty (“evidence is distorted or suppressed”), and dismissed the book as a whole as virtually worthless.

When I read the review I was dismayed. I know the book fairly well, since the period it covers is my own field of specialization. I personally did not agree with McDougall’s entire interpretation. But it was obvious from the book that he is an honest, serious, and highly professional scholar. It was abundantly clear, moreover, that an interpretation at least does exist. But this the reviewer apparently refused to admit. Certainly the reader could get no sense from the review as to what the overall argument of the book in fact is.

McDougall, incidentally, is not a personal friend of mine. Nor do I bear any grudge against Marks. And I do not particularly want to comment here on Marks’s specific allegations in any detail. Of the few I was able to track down, some were in fact correct (the Alsatian bishops); some were clearly false (Dulles’s status); and some apparently were gross distortions of the author’s meaning (the Little Entente). But the accuracy of the reviewer’s specific criticisms is really secondary. The important issue is that the general claims about the worthlessness of the book and the author’s lack of scholarly skill and integrity are grossly unwarranted. I feel it is important not to let so unfair an attack go completely un-

answered—or answered only by its victim, whose comments many readers may attribute to hurt feelings and dismiss in advance as self-serving.

It is not that anything I write buried here in the “Communications” section months after the review was published can possibly undo the damage the review has caused. But a certain standard of elementary fairness and decency needs to be upheld by the profession; and if no one speaks out such a standard would not exist even in theory.

MARC TRACHTENBERG
University of Pennsylvania

TO THE EDITOR:

We, along with other younger colleagues who have expressed their dismay to us, must report our distress at the harsh tone of Sally Marks’s review of Walter A. McDougall’s book, *France’s Rhineland Diplomacy, 1914–1924*, in the June 1979 issue of the *American Historical Review*.

Certainly a reviewer should point out where more documentation might alter conclusions, and he or she may contest the thesis of a book. Indeed, one of us has already debated aspects of McDougall’s conceptualization of post-1918 French diplomacy. Nonetheless, the assurance conveyed by the review that when archival material was not cited it was not known and when cited it was used only cursorily and, likewise, the reviewer’s recurrent examples from Belgian diplomacy to contest McDougall’s arguments about French policy—arguments moreover, that are never summarized for the reader—seem a less than fruitful critical approach. Without arguing that all books deserve mercy because they take work, we would suggest that reviewers keep a tone of criticism that recognizes that a common scholarly effort unites them with the author.

GERALD FELDMAN
University of California,
Berkeley
CHARLES S. MAIER
Duke University

PROFESSOR MARKS REPLIES:

In response to Walter A. McDougall’s letter, I acknowledge that additional substantiation of my contentions would indeed have been desirable, but unfortunately I could not move the *AHR* beyond seven hundred and fifty words, and thus I was unable to pack more evidence into my review. The same problem arises with this reply. Of course it is true that all books contain occasional errors, but McDougall’s massive inaccuracy far exceeds acceptable professional limits and destroys his credibility. This is particularly unfortunate because the topic of France’s Rhineland diplomacy is important, but

McDougall's book is much too flawed to contribute to our understanding of it. In addressing that situation, I focused primarily upon fundamental errors that seriously undermine the structure of the work. McDougall's rebuttal largely ignores these substantive matters.

My review's two sentences on Belgium seem reasonable, given the vital importance of Belgian policy both in their zone and on the High Commission to the fate of French-supported separatism. McDougall declares that Belgian policy initially was "military protection for Deckers' north Rhenish state, the key to Belgian strategy. The Belgian military, cabinet, and private intrigues all hoped to fashion a federalized autonomous Rhineland in which British and Belgian influence would balance the French" (p. 311). While two cabinet ministers and some junior officers supported Nothomb's intrigues—which did not contemplate British involvement—government policy was hostile from the outset. As Professor McDougall cites file 240, Jaspar Papers, he must have seen Theunis's sarcastic reaction, Nothomb's complaints of government hostility, and Jaspar's immediate declaration, "Les sottises dont nous avons tant pâti au moment de l'armistice vont recommencer." Jaspar added that he knew nothing yet about the separatist outbreak, that Nothomb's group had no mandate and had not consulted him, and that the Belgian government did not support its views. The resultant prompt collapse of the first 1923 separatist outbreak in the Belgian zone largely destroyed the credibility of subsequent French-backed separatism, but McDougall ignores this circumstance. Similarly, his depiction of the 1919 negotiation of the Rhineland Agreement as "refreshingly free of interallied conflict" (p. 83) deviates from both the documents and Keith Nelson's careful enunciation of them in *Victors Divided* (1975), which McDougall cites elsewhere in his work. Such disregard of data occurs frequently, although space does not permit additional elaboration of distortions previously noted on pages 197, 300, and elsewhere.

On reparations, Briand's statements to Lloyd George hardly constitute "what one clerk said to another." I complained, moreover, not of failure to recite minutes but of fundamental confusions, which I enumerated. Further, Briand did not accept the August financial division in December 1921 (p. 177); Professor McDougall confuses both the December scheme and its significance, which perhaps is why he ignores the confrontations at Cannes. Baldwin's brief meeting with Poincaré in September 1923 while passing through Paris and the exchange of vague speeches represented neither "a political event of the first magnitude" nor British concessions (p. 295).

I acknowledge with apology that brevity caused

imprecision concerning Dulles, but surely terming him an "unofficial envoy of the United States" and citing state department policy (pp. 288–89) implies inaccurately that he represented more than himself. Similarly, in the midst of two paragraphs (pp. 178–79) otherwise devoted to Lloyd George's attitudes at Cannes, Professor McDougall says not what he quotes above but rather that "the British cabinet expected the fall of " Briand. He adds, "Poincaré . . . might be easier to deal with." In context, this suggests Lloyd George's views, not poor paragraph construction.

I would gladly have discussed McDougall's thesis had I discerned one. As I indicated by example and quotation, there is so much muddle and contradiction in his book that one cannot discover where he stands. I did not lightly undertake the responsibility of writing a critical review but concluded that, if I did not, McDougall's fluent misinformation might be accepted by nonspecialists as "historical truth." Finally, I regret that my effort to warn readers of massive confusion strikes McDougall as unprofessional.

In further reply to Gerald Feldman, Charles S. Maier, and Marc Trachtenberg, I would remark that, if in fact Professor McDougall has read files that he neither lists nor cites, his suppression of evidence would exceed what I have suggested. Moreover, in exercising my scholarly judgment, fulfilling my obligation to readers of this journal, and writing the review that—alas—clearly had to be written, I certainly had no desire to become embroiled in the politics of the profession: that my rather euphemistic description of McDougall's scholarly deficiencies should generate such responses is indeed depressing.

Perhaps those who know Professor McDougall well have been able to ascertain his views through discussion with him. If so, it is a pity that none of them chose on this occasion to inform readers of his argument and its contribution. I, however, could only be guided by McDougall's book, which, as I indicated, sheds little light on his views because he frequently contradicts himself. Even if any consistent argument could be discerned, a further question would arise concerning the validity of an interpretation based upon such numerous inaccuracies. Certainly, greater fidelity to the documents and much more careful handling of evidence would have been required before this book could, in my judgment, qualify as a professional work of history. After all, a commitment to seeking the truth and investment of the necessary time and care in its pursuit are fundamental to scholarship. Without these, the historical fraternity could hardly claim to possess the standards requisite to a profession.

SALLY MARKS
Rhode Island College

Recent Deaths

At his retirement dinner in 1972 there gathered a wide assortment of people to congratulate RUSSELL LEON CALDWELL for his twenty-seven years of service to the University of Southern California and the Los Angeles community that surrounds the university. No one then realized that he would teach the history of the press in the School of Journalism for an additional five years and die on May 23, 1979, still an active member of the teaching faculty. Russell had the reputation through many years of being an inspiring, energetic, and witty teacher, and many of those who honored him at the retirement dinner were doubtlessly former students of the 1950s and 1960s who remembered his lectures in Bovard Auditorium before audiences of fifteen hundred people.

Russell Caldwell published amazingly little for a man who liked manuscripts, books, and literature. His only piece of writing, *American Freedom: A History*, was published in 1962, but earlier, before he began his professional career as a historian, he had published some articles on oratory and debating in the *Quarterly Journal of Speech*. Russell gave his energy instead to dissertations and theses, to lectures and commission reports, to university service in projects involving faculty government, AAUP services, and grievance committees, and to plans for the employment of retired faculty to improve the college.

Russell was born in the steel town of Farrell, Pennsylvania in 1904, attended high school there, and went to Hiram College in Ohio where he learned the art of debating and oratory, studied theology, and became interested in history. His early career was devoted to teaching in high schools in Pennsylvania and Ohio, where he won a reputation as a gifted speech coach who had regional and national champions. In 1933 he completed a masters degree from the University of Southern California in medieval history, but he was attracted to American history and returned in 1945 to the university to study U.S. constitutional history under Frank H. Garver. Before he completed the doctorate in 1948, he had been hired by the university to chair its Department of General Studies in Ameri-

can Civilization and awarded the rank of assistant professor. His facility in handling large student numbers brought advancement to associate professor in 1951 and professor in 1963. In those years, he had also taught as a summer faculty member at the Massachusetts Institute of Technology, the University of Michigan, UCLA, and Ohio State University.

A man of such energy is often difficult to characterize, but a sample of activities will illustrate his vitality. He wrote argumentative pieces for the *Daily Trojan*, often on teaching, sometimes on university policy, and many times on national issues; he sponsored petitions to the legislature against capital punishment; he sat on slum clearance boards and revealed the politics of the real estate lobbies; he spoke frequently in the university senate on tenure and salary matters.

His reputation is marked well by the careers of twenty or more successful doctoral candidates, fifty or more masters students, and the thousands of former students in the professions and businesses who benefited by his letters of recommendation. In 1972, one vice president of the university, in trying to capture Russell Caldwell's spirit, praised him as the most important member of the faculty in shaping the lives of undergraduates in the university. The audience rose, clapping, and toasted him for his devotion and loyalty to his students and the profession.

JOHN A. SCHUTZ
University of Southern California

JOHN F. GLASER, Professor of History at Ripon College, was killed in an automobile accident September 29, 1979. Born in Hamilton, Ohio, October 27, 1920, he attended Washington and Jefferson College, receiving his A.B. degree summa cum laude in 1941. He did his graduate work at Harvard University, earning an M.A. in history in 1942 and, after four years of military service (1942-46), a Ph.D. in 1949. He wrote his dissertation under the direction of David Owen. He taught at Washington Square

College of New York University from 1948 to 1954 but was on leave during 1952–53 to study in England on an American Council of Learned Societies fellowship. In 1954 he joined the faculty of Ripon College, rose quickly to the rank of full professor, and served as chairman of the history department from 1962 to 1969 and again in 1975–76.

In 1958 his article "English Nonconformity and the Decline of Liberalism" appeared in the *American Historical Review*; this same article was subsequently republished in two "problem books" dealing with English liberalism. A second article on "Parnell's Fall and the Nonconformist Conscience" appeared in *Irish Historical Studies* in 1960. He contributed more than fifty book reviews to the leading journals in his field.

Glaser was a charter member of the Conference on British Studies, was a member of its Executive Committee, and served as president of the Midwest Conference on British Studies from 1972 to 1974. He was a member of Phi Beta Kappa. Although his principal research interests were focused on Victorian England, he considered himself a humanist in the broadest sense of the word. His courses in European intellectual and cultural history received more and more of his attention in later years. Ideally suited to the academic environment of a small liberal arts college, he is remembered by two generations of students as a brilliant and witty lecturer and a stimulating if demanding instructor. His many friends and colleagues on both sides of the Atlantic will remember his warm hospitality and his absolute intellectual integrity.

GEORGE H. MILLER
Ripon College

MARGARET HASTINGS, Professor Emeritus of History at Douglass College, Rutgers University, died in an automobile accident on October 20, 1979, while on a visit to England. This is very sad news for many reasons but mostly because she had recently come through a period of poor health, and she and all of her legion of friends were looking forward to her enjoying a long, happy, and productive retirement.

Margaret Hastings was born in Springfield, Massachusetts, on May 23, 1910. She graduated from Mount Holyoke College in 1931, received an M.A. in English medieval history there in 1932 and a Ph.D. degree from Bryn Mawr in 1939, and supported herself in the meantime by teaching school. She continued to do this after 1939. Then, after a brief stint as a research analyst with the army late in the war, she joined the Douglass faculty as a lecturer in history in 1946, rising to the rank of full professor in 1959. She retired in 1975. She taught all sorts of courses during her years here, and all of them with the kind of perceptiveness and under-

stated wit that were the hallmarks of her conversation. As a teacher she was very good; as a scholar she was great. Her field was late medieval English law, a tangled thicket into which very few have the courage to venture. The records are voluminous, contradictory, and physically very awkward to handle; the language is often imprecise, and the handwriting ranges from difficult to unreadable. She ventured in and emerged with a book that was published in 1947 and became an instant classic: *The Court of Common Pleas in 15th Century England*, described by a British reviewer thirty years later, in 1977, as "one of the best books ever written on medieval England." She was not a rapid worker, and her bibliography is not a long one; but all her published work is first rate. Not all of it is narrowly specialized; her last book was an interpretive essay, *Medieval European Society, 1000–1450*, which, since it appeared in 1971, has been in regular use in Western civilization courses both here at Douglass and at many other colleges and universities. Her reputation among her professional colleagues was immense. English medievalists are a notably crotchety and contentious lot; but there was no dispute among them as to the quality of her scholarship. At various times in her career she was a Guggenheim fellow and a Fulbright fellow, and when the Helen Maud Cam visiting fellowship was established at Girton College, Cambridge, in honor of the great British medievalist, Margaret was its first recipient, a remarkable distinction for a colonial. It is pleasant to report that she was not without honor in her own backyard: she was the first Douglass College faculty member to receive a Lindback award for distinguished research, and in 1976 her alma mater, Mount Holyoke, gave her an honorary degree.

Margaret Hastings was a great scholar, with an enormously keen and perceptive mind—and, because she was modest, she never really took in how awfully good she really was. But she was much more than a great scholar. She was a wonderful colleague—who else would intervene in a tense personnel meeting with the suggestion that the rather erratic husband of one of her junior colleagues should be arrested so that that colleague could get on with her work? And she was a wonderful human being—warm and witty and wise, serious and funny by turns, and always worth hearing, whether the subject was hiking or Dickens, canoeing or Picasso, or medieval English law. The world, the profession, and all of her friends are the poorer for her passing.

MAURICE LEE, JR.
*Douglass College,
Rutgers University*

ALBERT HYMA, Emeritus Professor of History at the University of Michigan, died September 22, 1978,

in Marion, Ohio, at the age of 85. Throughout most of his life, he had been in vigorous health, until weakened after having abdominal surgeries during the Christmas holiday season in 1977 and in the spring of 1978. His death was caused by heart failure during surgery to replace a shattered hip.

Professor Hyma was born in Groningen in the northern Netherlands on March 18, 1893, and the years of his early childhood and youth were spent in Leeuwarden, to which his parents moved in 1896. When Albert was seventeen years of age, the family immigrated to America, where he attended Calvin College in Grand Rapids, Michigan, in 1913–14, and then the University of Michigan, at which he earned the B.A. in 1915, M.A. in German in 1916, and Ph.D. in history in 1922. During the school year 1916–17 he taught German at Knox College, in Galesburg, Illinois.

His career in history teaching began at the University of North Dakota, from 1922 to 24. In 1924 he returned to the University of Michigan, and there served until his retirement in 1962. During his tenure at Michigan, he also served as a guest professor at the University of Redlands during the school year 1944–45 and taught summer courses at various institutions, including the University of Chicago (1928), Ohio State University (1930), and the University of Colorado (1941).

Hyma's scholarly activity took him back to Europe as early as 1919–21, sponsored by the Algemeen Nederlandsch Verbond; and in 1928–29 he was in Europe again, as a Guggenheim Fellow. On one of several further trips abroad, he was knighted in 1936 by Queen Wilhelmina of the Netherlands for his scholarly work done in the field of Dutch history. His other scholarly honors and distinctions include election to fellowship in the Royal Historical Society in 1926, winning of the Henry Russell award at the University of Michigan in 1927, and election to fellowship in the Maatschappij der Nederlandsche Letterkunde te Leiden in 1957. He was also one of the founding editors of the *Journal of Modern History* in 1929, and he served as editor for the medieval and Reformation history areas in the *Twentieth Century Encyclopedia of Religious Knowledge*, published in 1955.

Near the beginning of his scholarly career Hyma gained almost immediate recognition as a Renaissance-Reformation specialist by his classic work, *The Christian Renaissance: A History of the "Devotio Moderna"* (1924), and he enhanced his reputation in this field by numerous further publications, including the textbook *Renaissance to Reformation* (1951, 1955) and various biographical studies on Erasmus, Luther, and Calvin. His more than forty published books and several hundred articles and book reviews reveal, however, interests and expertise beyond the Renaissance-Reformation field. With

Arthur E. R. Boak and Preston Slosson he co-authored the standard history textbook *The Growth of European Civilization*, two volumes, first published in 1936 and appearing in later editions in revised form as *The Growth of Western Civilization*; and he authored or co-authored further general history textbooks in later years. As an expert in the history of the Dutch in the Far East, he produced *A History of the Dutch in the Far East* (1953), as well as *An Outline of the Growth of Far Eastern Civilizations* (1946). His interest in the Dutch in America and in early Michigan history led him to produce his *Albertus C. Van Raalte and His Dutch Settlements in the United States* (1947) and to co-author with Frank B. Woodford the book *Gabriel Richard: Frontier Ambassador* (1958). His breadth of knowledge and expertise was also evidenced in the numerous book reviews that he provided for both American and European publications, treating a wide array of works with titles in seven different languages (some fifty reviews have appeared in the *American Historical Review*).

At the time of Hyma's retirement from the University of Michigan, he was honored not only by the university but also by some fifteen of his doctoral graduates and other friends who presented him with a festschrift volume entitled *The Dawn of Modern Civilization: Studies in Renaissance, Reformation and Other Topics Presented to Honor Albert Hyma* (1962, 2d ed., 1964). His formal retirement did not, however, mean inactivity for him. In addition to further writing and general lecturing, he accepted semester- or year-long teaching appointments at such institutions as Aquinas College in Grand Rapids, Michigan; Western Kentucky University in Bowling Green, Kentucky; and Christian Heritage College in San Diego, California.

The year 1927 had been a most important year in Hyma's life, for in that year he married Vera Alberta Nodine, a marriage that resulted in a happy home with three children and in a companionship of mutual interest in his historical studies. The year 1927 was indeed doubly significant for him, for it was also in that year that he was naturalized a United States citizen. Shortly after his retirement in 1962, the Hymas moved to Holland, Michigan, where they made their home until 1976. Since then, they resided near their children, first in Brigham City, Utah; then in Schenectady, New York; and lastly in Marion, Ohio. After Professor Hyma's death, Mrs. Hyma returned to Holland, Michigan, in November 1978.

Hyma achieved recognition as both a church historian and a secular historian, a dual role in which he displayed a remarkable capacity for combining divergent elements—the sympathetic understanding of one who lives genuinely and experientially within the framework of the church and the scholarly technique of one who is absolutely and thor-

oughly devoted to the strictest standards of the historical discipline. A church member reared in the Reformed tradition, he was committed to the Christian way of life. A disciplined historian, he was dedicated to the search for historical truth. The two facets of his experience never clashed. Rather, he combined them in beautiful harmony—a harmony that united the warmth of one who feels with the authority of one who knows.

But Professor Hyma's true greatness reached beyond the realm of scholarship. His personal magnetism, his genuineness of character, his sincere and friendly demeanor—these were among the noble qualities that endeared him to those of us who had the high privilege of personal association with him. He was a great scholar and a great teacher, but he was also a great Christian gentleman and a great friend.

KENNETH A. STRAND
Andrews University

BERNARD MAYO, Emeritus Professor of History at the University of Virginia, died on August 20, 1979, in Charlottesville, Virginia. Born at Lewiston, Maine, on February 13, 1902, Mr. Mayo attended the University of Maine, received his A.B. degree in 1924 and A.M. degree in 1925 from George Washington University, and earned his Ph.D. at Johns Hopkins University in 1931. He taught at the National University from 1926 to 1937 (it merged with George Washington University in 1954) and at neighboring Georgetown University from 1937 to 1940. In the fall of 1940 he moved to the University of Virginia where he taught until his retirement in 1972. At that time his appreciative former students gathered and presented him a festschrift, *America: The Middle Period: Essays in Honor of Bernard Mayo* (1973). Mr. Mayo served as visiting professor at Harvard in 1946–47 and taught summer school at Columbia (1946) and the University of Missouri (1961). In 1958 he gave the inaugural Eugenia Dorothy Blount Lamar Memorial Lectures at Mercer College, published as the delightful *Myths & Men: Patrick Henry, George Washington, Thomas Jefferson* (1959). More than twenty years before, Mr. Mayo had begun his scholarly career with his distinguished *Henry Clay: Spokesman of the New West* (1937, reprinted 1966). He edited for the American Historical Association *Instructions to the British Ministers to the United States, 1791–1812* (1941) and followed that with *Jefferson Himself: The Personal Narrative of a Many-Sided American* (1942). Like Jefferson himself, Mr. Mayo's writing demonstrated "a particular felicity of expression." Active in his profession, Mr. Mayo was a member of the American Historical Association, the Organization of American Historians, the Society of

American Historians, the Southern Historical Association (member of the executive council, 1942–44, and of the editorial board, 1944–47), the Virginia Social Science Association (executive council, 1954–57, vice-president, 1959–60, president, 1960–61), and the Albermarle County Historical Society.

For two generations of students Mr. Mayo's superb teaching was his most memorable achievement. With unfailing charm and wit he taught them how to think more clearly and write more gracefully. He had that special ability to inspire students to do better than their best. One of his most characteristic accomplishments was the founding of the History Club at the University of Virginia in 1948. For many years the home of Professor and Mrs. Mayo was the center of things at the university for townspeople, visiting dignitaries, faculty members, and, most of all, for hundreds of students. Mr. Mayo was a man of liberal sentiments and cultivated tastes. He loved his music, his books, and his garden. He could light up a room with his presence. It was altogether appropriate that at the University of Virginia's fall 1976 convocation he gave the feature bicentennial address, "Another Peppercorn for Mr. Jefferson" (published in 1977). Again like Jefferson, Mr. Mayo was a man for all seasons who enriched immeasurably the University of Virginia and all whose lives he touched.

Mr. Mayo is survived by his wife, Margaret McClure Macleod, two children, Dr. Malcolm Macleod of Sacramento and Margaret Macleod Kern of Chicago, and several grandchildren.

W. W. ABBOT
University of Virginia
JOHN B. BOLES
Tulane University

After a long fight with cancer, PHILIP P. POIRIER, Professor of History at the Ohio State University, died at Columbus on February 28, 1979, at the comparatively early age of fifty-eight. He was one of a brilliant group of Harvard graduate students who worked in the late 1940s and early 1950s with David Owen and have done so much to reshape the study of Britain in the United States since the Second World War, helping to give it dramatic new dimensions. Poirier belonged to a new generation of academics who were less traditionally minded and less traditional in background than many of those who had entered academic life previously, particularly in the area of British studies. He was born and grew up in Gloucester, Massachusetts. He graduated from Boston University in 1942 (and at the university's Centennial Celebration he was cited as one of its 100 most distinguished alumni). Then he went off to war, served in the army in both France

and Germany, and was awarded the Bronze Star. At Harvard he received his M.A. in 1948 and his Ph.D. in 1953.

Poirier made his special area of concern the growth of the Labour Party: the way it attempted to transform British society at the same time that it drew deeply from lasting British traditions, so that it is the despair of the more doctrinaire (and at times of the more adventuresome) but also the hope of those who believe that the left can have its roots and traditions. In private conversations and in papers and comments given in London, New York, Washington, and elsewhere at various conferences and meetings, Poirier was tough-minded and thorough, had a total command of the sources, most particularly those to be found in private papers, and would not tolerate sloppy thinking in himself or in others. He was an example of how scholarship should be conducted—he was unfailingly polite and unfailingly insistent on getting things right.

His major work, *The Advent of the British Labour Party* (1958), showed all his skills at their most impressive. Two British scholars and he were the first to take advantage of newly opened papers that revealed the arrangements made by the Labour Party with the Liberal Party not to fight one another in constituencies in so far as that was possible. It was a plan that allowed the Labour Party to return a significant number of M.P.s in the General Election of 1906. Poirier's study, dealing with the six crucial years from 1900 to 1906, is extraordinary in its range of research and is written with elegance and force. It is a model monograph. Dealing with an important subject, it changed the course of thinking about a crucial period. Poirier created a book that has lasted and will continue to be read by all students of the Labour Party and Britain. Its position was recognized in 1961 when it received the first triennial prize of the Conference on British Studies. He continued, through numerous reviews and talks, to elucidate aspects of modern Britain and provide directions, suggestions, and insights for his fellow scholars in both the United States and Britain. In 1974 he wrote an important introduction for a reissue of L. T. Hobhouse's *The Labour Movement*, and at the time of his death he had finished the research and was about to embark on the writing of a study of "The Impact of Conscription in Great Britain during World War I." It is a great loss that we are deprived of this study, which would have combined his acute sense of the politics of the period with an awareness of the social dimensions of conscription. (There are hopes that another scholar will be able to bring his work to completion.) Poirier was a considerable figure in the international world of scholarship and his pungent and powerful contributions will be sorely missed.

Even as his absence will be much regretted in the world of British studies, and by his many friends in

the profession, so too will he be missed at Ohio State, where he spent his entire teaching career, other than a brief experience as an instructor at Williams College in 1952. That same year he came to Ohio State as an instructor. He moved up the ranks, becoming a full professor in 1964. There too he met his wife, Carole Rogel Poirier. He taught a full range of courses, primarily on modern Britain at the graduate and undergraduate levels, but in fact the last course he taught, in the autumn of 1978, was on nineteenth-century European intellectual history, one of his regular offerings, suggesting the breadth of his mind and interests. He was deeply involved with graduate education, and while he was on various committees for his department and at the university, probably his most notable service was in the reform of the history graduate program. As a colleague has written, "His own Seminar for M.A. and Ph.D. students was a model of its kind—well organized, exacting, demanding, yet not excessively severe. He was renowned for strengthening the graduate program and for upholding and setting standards." He himself firmly believed in the need to study manuscript collections in England, and during his career received support from the American Council of Learned Societies, the American Philosophical Society, and the Guggenheim Foundation in order to do so. (His position was also recognized in his election as a Fellow of the Royal Historical Society.) It is particularly appropriate that the memorial to him should be a fund designed to enable graduate students to do research in England. (Contributions to it should be sent to Professor Gary Reichard, Chairman, Department of History, 230 West 17th Avenue, The Ohio State University, Columbus, Ohio 43210. Checks should be made out to the Ohio State Development Fund and marked the Philip Poirier Memorial Fund.)

British studies, most particularly the investigation of the period of the early part of this century, when so much of what we now recognize as the modern world came into being, have suffered a great loss in the death of Philip Poirier.

PETER STANSKY
Stanford University

MORRIS FENTON TAYLOR, Professor of History and Head of the Social Studies Division at Trinidad State Junior College in Colorado, died of cancer on January 29, 1979. He was sixty-three years old at the time of his passing.

Born in Mount Morris, New York on October 21, 1915, Morris Taylor spent his youth in Warsaw, New York. In 1939, he received his B.A. from the University of Colorado, and in 1940 he obtained his M.A. from Cornell University. In 1941, he did advanced graduate work at Cornell, and in September

of 1941, he joined the faculty at Trinidad State Junior College. Except for serving in the United States Marine Corps from 1942 to 1943, Morris Taylor remained a member of the faculty at Trinidad State for nearly forty years. In 1969, the University of Colorado awarded an honorary Doctorate of Humane Letters to him.

Elected to a three-year term in 1974 to the Executive Council of the Western History Association, Morris Taylor's other officerships included President of the Colorado Federation of Teachers, President of the Colorado Archaeological Society, President and then life-time Honorary President of the Trinidad Historical Society, President of the Colorado History Group, President of the Community Concert Association, Vice-President of the Colorado Conference of the American Association of University Professors, and Regional Vice-President of the State Historical Society of Colorado. His professional affiliations included membership in the Organization of American Historians, the Western History Association, the State Historical Society of Colorado, the New Mexico Historical Society, the Great Plains Historical Association, and the Trinidad Historical Society.

Morris Taylor won numerous awards and honors, both while living and posthumously. He participated in the Fulbright Teacher Exchange and taught in Kent, England in 1960 and received a fellowship to study at the Huntington Library in 1976. The American Association for State and Local History presented him an award in 1967 for his book entitled *Trinidad, Colorado Territory*, and five years later the same body awarded him a commendation for his research and publications on topics concerning the Southwest. He received the Historical Society of New Mexico Certificate in recognition of his writings on New Mexico history in 1971, and he won the LeRoy R. Hafen Award from the State Historical Society of Colorado for the best article published in *The Colorado Magazine* in 1974. Memorial awards included the establishment of the Morris F. Taylor Memorial Scholarship to Trinidad State Junior College, the dedication of two issues of *The Colorado Magazine* to his memory, and the establishment of a Morris F. Taylor Library by the Trinidad Historical Society in the Trinidad Museum Complex.

As both a teacher and a scholar Morris Taylor excelled. Although teaching four or five courses per term, during his career he still published six books, two of which being *First Mail West*, published by the University of New Mexico Press in 1971, and *O. P. McMains and the Maxwell Land Grant Conflict*, published by the University of Arizona Press in 1979. His numerous articles appeared in several historical journals.

Professor Morris F. Taylor will certainly be

missed. Fortunately, his writings and the several memorial awards he received will remind us of his contributions to the field of history.

RAYMOND WILSON
Fort Hays State University

T. HARRY WILLIAMS, Boyd Professor of History at Louisiana State University, died on July 6, 1979, in Baton Rouge, Louisiana. He was admired and held in deep affection by a multitude of students, former students, colleagues, and other acquaintances. This year was his retirement year; he was honored in appropriate ceremonies at his university only about two months before his death.

Professor Williams was born May 19, 1909, in Vinegar Hill, Illinois. He grew up and received his education in Wisconsin, where he gained a bachelor's degree from Platteville State Teachers College in 1931 and a master's degree from the University of Wisconsin in 1932 and the doctorate in 1937. He taught in the extension division of the University of Wisconsin from 1936 to 1938 and at the University of Omaha from 1938 to 1941, the year he came to Louisiana State University. He became a Boyd Professor in 1953. He was a visiting professor at various universities; he was the Harmsworth Professor of American History at Oxford University during 1966-67. He was a member of the American Historical Association throughout most of his career. He was also a member of the Southern Historical Association, serving as president in 1958-59, and of the Organization of American Historians, serving as president in 1972-73. He received honorary degrees from five colleges and universities.

Professor Williams was a highly productive and distinguished scholar. He wrote or edited some 20 books, and he wrote dozens of articles and book reviews. He directed the work of 36 students who earned the Ph.D. degree and 57 who earned the M.A. degree. The subject that engaged him over the longest period of his professional life was the American Civil War; his most impressive books in this field were *Lincoln and His Generals* (1952), which was a Book-of-the-Month Club selection, and *P. G. T. Beauregard: Napoleon in Gray* (1955), which some reviewers believed was worthy of a Pulitzer Prize. At the prime of his career he turned his attention to the twentieth century. In 1969 he brought out *Huey Long*, which won both the Pulitzer Prize and the National Book Award. At the time of his death he was at work on a biography of Lyndon Baines Johnson.

Professor Williams was as outstanding in the classroom as at his desk. He was essentially an old-fashioned teacher. He dealt brilliantly with political, economic, and social causes and effects, but his deepest interest was in people as individuals. He delivered sharp analyses and concise syntheses of ma-

jor historical developments, but his favorite method of instruction was by narration and anecdote. With an encyclopedic command of the facts, an unexcelled vividness of expression, and an almost matchless sense of both pathos and humor, he made his courses into high theater.

He was no mere chronicler of events. His writing and teaching were an elaboration of his reflections upon the lessons of the past. He was fascinated by strength of personality and by the exercise of power in the affairs of society. He believed, up to a point, in the Great Man theory of history: that social movements do not simply occur but are the result of personal inspiration and leadership. This conviction pervades his works on Lincoln and Long, both of whom he admired in spite of the obvious great differences between them, and the belief shows clearly throughout his other writings, as it did in his lectures. He was captivated also by the colorful personality, whether effectual or ineffectual. His biography of Confederate General Beauregard, a resourceful but visionary and erratic Louisiana Creole for whom he had a genuine fondness, is a study in the exotic and the romantic. Beauregard, he wrote, "had more glamor and drama in his Gallic-American personality than any three of his Anglo Saxon colleagues in gray rolled into one." In a more casual and more pungent idiom Professor Williams said that Beauregard in the midst of the Lees, Johnstons, and Jacksons of the Confederacy was like a serving of shrimp jambalaya surrounded by a mess of turnip greens.

Professor Williams was as interesting outside the classroom or study as he was inside them. After moving to Louisiana he quickly adopted the style of the bayou state. He married an accomplished and gracious Louisiana lady, Estelle Skolfield, and he became an inseparable part of the local scene. Physically spare, he more than compensated for his lack of robustness with the force of his intellect and the readiness of his wit. Activated by an inexhaustible nervous energy, he lived at the height of mental and emotional tension. A master of the sharp comment and the instant retort, he enjoyed nothing more than a verbal tilt with a colleague or graduate student. Yet he was a warm and generous man. His home was the center of frequent social gatherings where faculty and graduate students mingled freely and exchanged opinions and gibes with startling candor. His death put out one of the brightest lights of the academic community.

The subject of Professor Williams's most celebrated book, Huey P. Long, once said of himself that he was *sui generis*—the only one of his kind. This is perhaps a fitting characterization of T. Harry Williams.

CHARLES P. ROLAND
University of Kentucky

EDWARD E. YOUNGER, Alumni Professor of History at the University of Virginia, counselor and friend to hundreds of graduate students, died in Charlottesville, June 23, 1979. He had retired on May 31 after more than thirty years of dedicated service to the Corcoran Department of History, the university, and the profession.

A native of Arkansas, Edward Younger began a distinguished teaching career in a one-room school at the age of sixteen. While teaching full time, he earned a bachelor's degree from Arkansas State College. Leaving a high school principalship for the uncertainties of graduate study in the depression years, he took the M.A. in history from Oklahoma A & M College in 1938 and the Ph.D. from George Washington University in 1942. After completing a tour of duty in the navy in World War II, he was appointed assistant professor of history at the University of Virginia.

His contributions to the university over the next three decades were manifold. He developed courses in American diplomatic history, the Gilded Age, and twentieth-century United States, and, despite the increasing specialization of the profession, continued to teach them for more than twenty years. As chairman of the Corcoran Department of History from 1962 to 1966, he presided over a program of expansion that nearly doubled the size of the faculty and attracted a number of distinguished historians to the university. From 1966 to 1969, he served as dean of the Graduate School.

In an era when scholarship flourished as never before and publication was the standard means of professional recognition and advancement, Edward Younger's scholarly achievements seem modest. His biography of John A. Kasson, Iowa politician and diplomat of the Gilded Age, nevertheless remains a model study of a second-level political figure, and his many essays and reviews are distinguished by precision of analysis and clarity of style.

It was characteristic of the man that he sacrificed his own scholarly work to the service of others. Like many of his generation, he was deeply sensitive to the new role being assumed by the United States in the world, and he sought to employ his academic expertise to further international cooperation. As Fulbright lecturer at Allahabad University in India in 1957-58, he worked diligently and patiently to explain American history and foreign policy to frequently skeptical audiences. He returned to the United States with a keen interest in Indian history and culture and a determination to increase American knowledge of and friendship for the world's largest democracy. He lectured extensively on India in Virginia and across the nation and was instrumental in establishing a South Asian studies program at the University of Virginia.

While encouraging the study of different cultures,

he also maintained a special scholarly interest in his adopted state. Throughout his career, particularly in the last ten years, he enthusiastically promoted the study of recent Virginia history, an area largely neglected by a people steeped in the antebellum past. He urged graduate students to work on post-Civil War Virginia and played a major part in procuring the manuscripts that served as their raw materials. After completing his term as graduate dean in 1969, he launched, in collaboration with his students, two ambitious projects now nearing completion, a collection of biographical essays on the post-1865 governors of Virginia and a history of Virginia after the Civil War. For his pioneering work in this field, he was named an Honorary Fellow of the Virginia Historical Society in 1973.

Edward Younger will be most affectionately remembered as a graduate teacher. By the time of his retirement, he had directed 65 dissertations and had supervised the work of 140 M.A. candidates. The numbers are staggering, but there was never the slightest resemblance to a "Ph.D. mill." A demanding taskmaster, he oversaw each project with painstaking care and insisted on the highest standards of scholarship. He gave generously of himself to all of his students. Even during the time he was department chairman and graduate dean, he was available day and night for assistance, counsel, and, when necessary, ego-stroking. Considering the rigors

of his early career, it must have been difficult for him to sympathize with those of us who were at least slightly spoiled by an age of unparalleled prosperity and unique academic opportunity. Yet he somehow understood. He kept countless people in school when they were inclined to drop out. He drove through to completion many dissertations that might have languished. His interest continued after students departed from the university. If they were dissatisfied with positions, he helped them find new ones. When they encountered professional frustration, he listened patiently and with that knowing twinkle in his eye put things in perspective. He took enormous pride in their accomplishments, always in a completely selfless manner. Those who returned for visits found the door open, as before, and the same warm hospitality and rapt attention.

In the words of colleague Martin Havran, Ed Younger, above everything else, was an "intensely humane man." Quiet and unassuming, always reserved and dignified, he radiated warmth and kindness and possessed a sly sense of humor that he employed with a light and unerring touch. He was a master at getting the best from those who worked for and with him, seemingly without effort. He will be deeply missed and long remembered by those who knew and loved him.

GEORGE C. HERRING
University of Kentucky

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MEETINGS: The Association's annual meeting takes place on December 28-30. The meeting in 1980 will be held in Washington, D.C. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

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CORRESPONDENCE: Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

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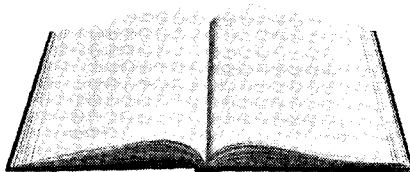
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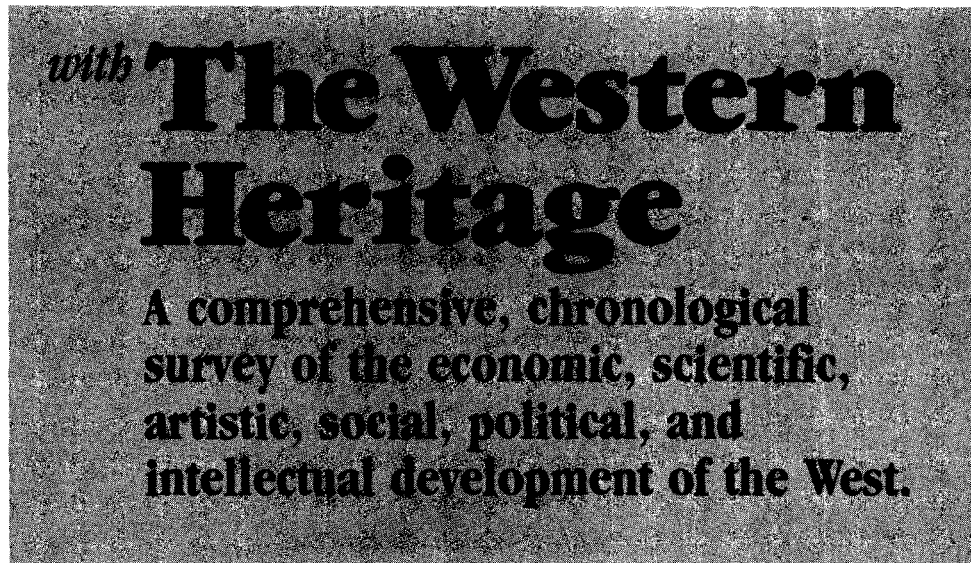
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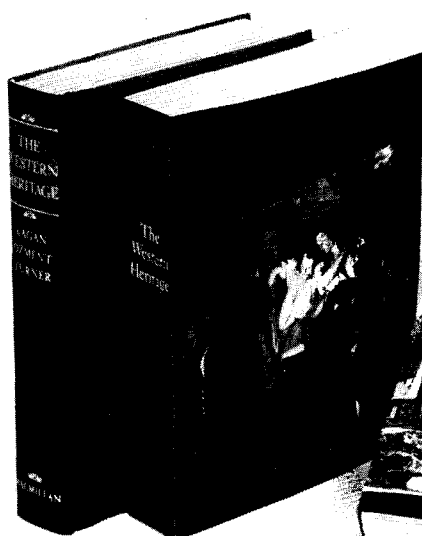
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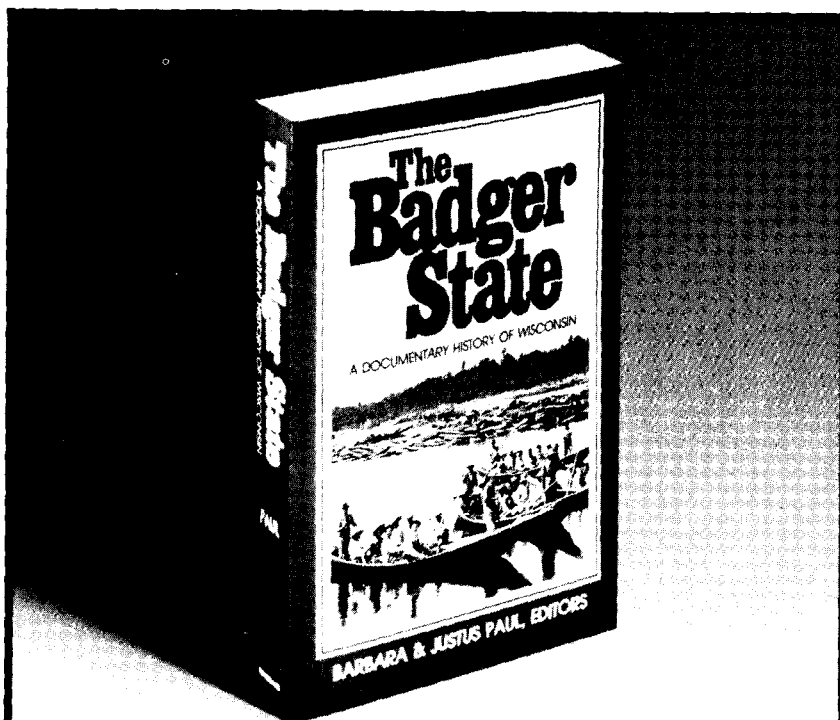
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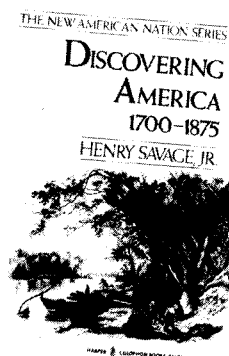
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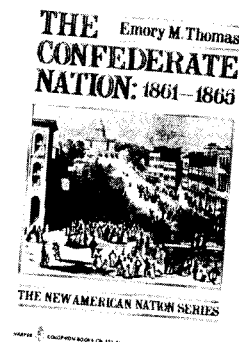
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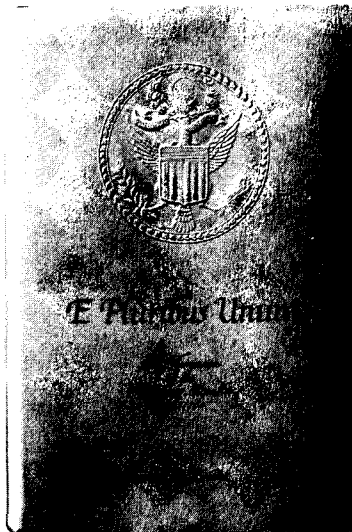
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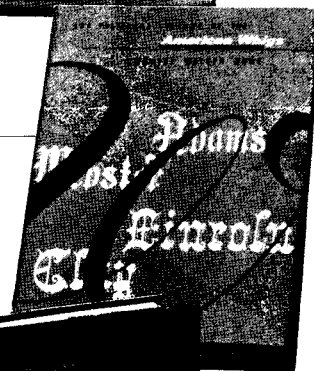


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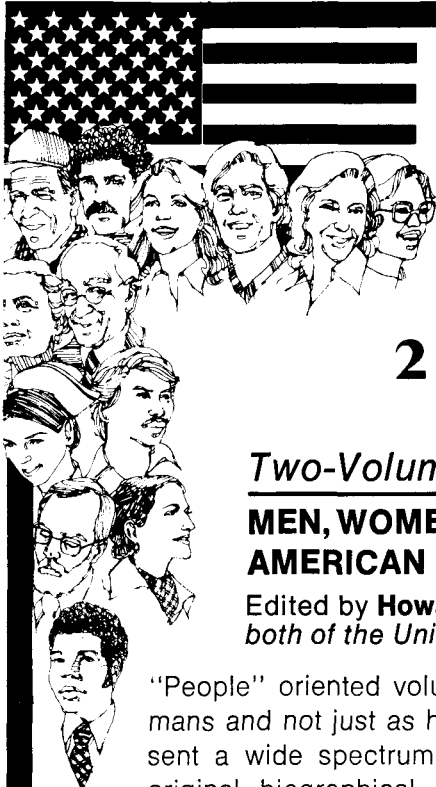
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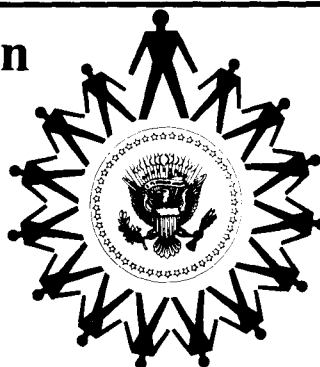


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